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EDITED BY
†HOWARD C. WARREN, Princeton University

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The Psychological Review

ON EMOTIONAL EXPRESSION AFTER DECORTICA-TION WITH SOME REMARKS ON CERTAIN THEORETICAL VIEWS [1]

PART I

BY PHILIP BARD

Johns Hopkins School of Medicine

It is a well established fact that a decorticate cat or dog, in the chronic condition, is capable of displaying a type of behavior which is commonly regarded as expressive of anger or rage. After removal or disconnection of all parts of the cerebral cortex and with injury to corpora striata and the dorsal part of the diencephalon, Goltz's dog (17) lived in good health for more than eighteen months. Emotional expression in this instance was apparently confined to a reaction consisting of barking and growling, with vigorous attempts to bite. The response was regularly elicited in stereotyped form by such procedures as lifting the animal from its cage. The decorticate dog described by Rothmann (34) showed a similar mode of response: snarling and growling were obtained by gentle scratching of its back, and the presence of a fly on the creature's nose was observed to induce a fit of activity which closely resembled the picture presented by an enraged normal dog. When disturbed in various ways the two decorticate cats prepared and studied by Dusser de Barenne (12)

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displayed energetic movements of defense and those reactions so characteristic of the angry cat—spitting, growling, erection of hair.   In the decorticate cat described by Schaltenbrand and Cobb (36) the writer observed that light pinching of its tail regularly evoked hissing and biting with lowering of the head, arching of the back and vigorous clawing of the surface upon which the animal rested.   Finally, it may be said that in the course of the last three years I have prepared and studied, during long survival periods, four decorticate cats and three decorticate dogs.   A detailed description of the types of emotional response exhibited by these animals will be given in Part II of this paper.   For the present it will suffice to say that my observations have confirmed those of the earlier investigators and have convinced me that it is correct to assert that a chronically decorticate cat or dog can *express* rage.

I. The Sham Rage of the Acute Decorticate Cat and its Significance

The observations of Goltz, Rothmann and Dusser de Barenne definitely indicated that the expression of rage is not wholly dependent upon cortical activity, but may be the result of a motor discharge of subcortical origin.   Furthermore, their descriptions gave the impression that removal of the cerebral cortex renders an animal unusually disposed to exhibit the rage reaction.   It was this feature of the earlier work that suggested to Professor Cannon the possibility of using an acute decorticate preparation for the study of the emotional activation of the sympathetic division of the autonomic system.   In 1925 he reported with Britton (10) that after disconnecting the cerebral cortex from the brain stem there appears in cats, on removal of the anesthetic (ether), 'a group of remarkable activities such as are usually associated with emotional excitement—a sort of sham rage.' The behavior exhibited by the animals in these acute experiments closely resembled the behavior of an infuriated normal animal.   The motor activities of sham rage are both somatic and visceral.   Struggling, clawing, snarling, lashing of the tail, hyperpnea and panting are combined with all the signs

of widespread and vigorous activity in the sympathetic system: erection of hair, dilatation of the pupils, retraction of the nictitating membranes, exophthalmos, profuse sweating from the toe pads, and enormous increments in arterial blood pressure and heart rate together with evidence of discharge of adrenin from the adrenal medulla.

The Delimitation of the Source of the Activity.—The investigation to which reference has just been made gave further support to the view that the bodily changes which constitute the expression of rage have their origin in the brain stem. Bard therefore undertook the experimental delimitation of the subcortical region responsible for the sham rage of the decorticate cat. The results of this investigation were presented in great detail in the original paper (1) and quite amply in two subsequent publications (2, 3). But since the experimental results themselves as well as certain conclusions drawn from them by Cannon and by Bard have been misinterpreted or incorrectly related to other data (18, 19), it may serve to prevent future misunderstandings if a precise restatement of the findings be made at this time.

The experimental method adopted in that investigation was the ablation of varying portions of the brain stem after removal of the cortices of both cerebral hemispheres. In the series of forty-six successful acute experiments it was found that the sham rage occurred regularly after ablation of corpora striata and the rostral half of the diencephalon [experiments 4, 5, 26 and 34 (1)]. It appeared in typical form after still more caudal truncations of the brain stem. For example, the cat of experiment 13 exhibited the most intense activity encountered in the entire investigation, and yet the transection was one that left only a thin caudal segment of the diencephalon. This consisted, ventrally, of the distal portion of the hypothalamus and, dorsally, of a correspondingly small amount of thalamus, epithalamus and metathalamus. That the dorsal portion of this segment was not concerned in the activity was shown quite conclusively by the results of experiments 21, 37 and 44, in which the sections passed ventrally and rostrally from the upper part of the anterior colliculi.

In these animals there remained rostral to the midbrain only the greater part of the hypothalamus and small ventrocaudal fractions of the thalamus. When the brain stem was transected at the caudal extremity of the diencephalon (No. 40, and six similarly prepared animals) or through the cranial portion of the mesencephalon (*e.g.*, experiments 46 and 31), the sham rage invariably failed to develop.

From these results the conclusion was drawn that the discharge of nervous impulses, which evokes the extraordinary motor activity of the acute decorticate preparation, is conditioned by central mechanisms which lie within an area comprising the caudal half of the hypothalamus and the most ventral and most caudal fractions of the corresponding segment of the thalamus. From the point of view of localization the experimental facts justified a somewhat less conservative statement. To the reader of the original publication, who possesses a knowledge of neuro-anatomy which would permit a perusal with understanding, it should be clear enough that the work gives only the barest suggestion that any part of the diencephalon other than the distal hypothalamus is concerned in the sham rage.

Thalamus and Hypothalamus.—In view of the fact just stated, it is somewhat surprising to find Harlow and Stagner (**18**) making unrestrained use of my work to support their idea that the thalamus contains an 'excitement center.' They write, "The existence of an 'excitement' center in the thalamus finds objective support in the experiments of Bard, who obtained a reaction of strong 'emotional' excitement in the cat (46 successful observations) following ablation of the hemispheres, corpora striata and the cranial half of the diencephalon. Four animals showed the 'sham rage' following ablation of the dorsal half of the diencephalon, although the skeletal muscle components of the responses were somewhat limited, and spontaneous excitement did not appear as readily as in the less severe operations." From this it appears that in searching for such data as seemed favorable to their hypothesis these writers overlooked those experiments in which far less than the caudal half of the diencephalon

remained, and directed their attention to two of the four preparations in which activity followed sections that, in passing forwards and downwards from the anterior colliculi, cut away all but very small ventral and caudal fractions of the thalamus (in no instance was there any 'ablation of the dorsal half of the diencephalon'). They evidently refer to the results of experiments 21 and 37, which I summarized by writing, "In short there seemed to be a preponderance of sympathetic over cerebro-spinal discharge." But they failed to take into consideration the results of experiment 44 (presented in detail in Table 4) in which, after the same ablation as was performed in 21 and 37, the skeletal muscle components of the activity were as great as in other positive experiments in which all or a very large part of the thalamus was left intact. It would be a rash neuro-anatomist who would venture the suggestion that the small fragments of thalamus remaining in experiments 13, 21, 37 and 44 were of any functional significance. It is therefore reasonable to conclude that Harlow and Stagner have been definitely incautious in turning to my results for substantiation of their claim that the thalamus [2] is the center for what they term the conscious and reflex expression of the feeling of excitement. Here the issue simply involves a matter of anatomical understanding and a regard for evidence. It is not my intention to deny that the thalamus *may* play a rôle in the development of a state of 'excitement,' but it is my wish vigorously to deny that my experiments give to that possibility the support claimed by Harlow and Stagner. And a little further on I will have something to say of their contention that what Cannon and I have called 'sham rage' is in reality what they call 'excitement.'

That the hypothalamus rather than the thalamus contains the central mechanism responsible for the sham rage is strongly suggested by a certain group of facts which have been

[2] Reliable information concerning the nuclei, boundaries and connections of the thalamus, together with accounts of its relation to the rest of the diencephalon, can be found in many standard works on neurology and anatomy. The papers by Rioch (31, 32, 33), Ingram, Hannett and Ranson (23) and the Atlas of Winkler and Plotter (38) contain authoritative accounts of the feline diencephalon.

discussed by Bard in each report that he has made on this subject. They need not detain us long here. Suffice it to say that, although various tonic and reflex sympathetic discharges are managed by bulbo-spinal mechanisms (1, 2, 37), it is clear (*a*) that practically all parts of the sympathetic outflow can be made to discharge by localized hypothalamic stimulation (6, 15, 21, 22, 26, 30, 37) and (*b*) that this result cannot be induced by similar stimulation of the thalamus (26, 30, 35). What is the significance of the first fact? Cannon (7) has pointed out that the sympathetic system is organized to act as a unit and that it discharges as a whole under conditions of physiological stress. This suggests that superimposed on the bulbo-spinal sympathetic centers is a dominant central mechanism responsible for the sympathetic activity characteristic of emergency states. Conspicuous among the latter are emotional excitement and exposure to cold. The actual existence of such a center and its hypothalamic location are supported not only by the work on sham rage, but by the various lines of work which have delimited the part of the brain essential for regulation of body temperature (24, 25, 28).

Dusser de Barenne and Sager (14) have recently published some results that emphasize the entirely distinct relationships which thalamus and hypothalamus have to the expression of emotional excitement of the rage type. Since their work will later be referred to in another connection it will be convenient to describe it in some detail. Some time ago Dusser de Barenne (11) showed by means of his excellent method of local strychninization that there is in the cat's cortex a large sensory zone which is divisible into foreleg, head and hindleg areas and a zone of 'crossed symptomatology.' The application of strychnine to any very small fraction of one of these areas gave rise to signs of intense sensory excitation referable to the corresponding part of the body and consisting of (*a*) symptoms of spontaneous excitation, paresthesiæ, (*b*) hyperesthesia and hyperalgesia, and (*c*) exaggeration of Munk's *Berührungsreflexe*, which are cortical reactions. Except in one of the thirty-four experiments, no genuine

symptoms of motor excitation were observed. Later, very similar results were obtained in the monkey (13). Now, with Sager, Dusser de Barenne has found, in an admirable study involving 184 cats, that the local application (by injection) of strychnine to various parts of the thalamic nuclei gives a symptomatology practically identical with that obtained in the cortical experiments. The only difference lay in some signs of irradiation which were present in some of the thalamic experiments but did not show up in the earlier work. This may be explained by the fact that the representations of the several body regions are somewhat mixed and much less sharply separated in the thalamus than they are in the cortex. On the basis of the published protocols, the typical behavior of a cat under the influence of localized thalamic strychninization can be indicated as follows:

If the injection is into the *left* thalamus and affects the sensory representation of the *forelegs*, the animal will usually show signs of spontaneous sensory disturbances, paresthesiæ, in the forelegs, more marked in the right than in the left. The paw is repeatedly lifted and put down carefully and slowly; it may be suddenly shaken, then licked, and finally the skin may be grasped between the teeth and angrily shaken. Though spontaneous, such actions are intermittent and, when the animal is free from them, evidence of hyperesthesia and hyperalgesia is obtainable. If the leg be lightly touched it is shaken and the spot stimulated may be viciously bitten. It will then be held up, licked, and put down gingerly. Subsequent attempts to stimulate are likely to be forestalled by active movements of defense, by striking and biting with spitting. But if the affected area is again touched the animal will turn upon the stimulated spot, tear at the skin, shake it and wound itself. Deep sensibility is also affected, but the disturbance is wholly contralateral. There is hypersensitivity of the skin not only for touch, but for pain and temperature (both heat and cold).

Quite distinct from the cortical and thalamic symptomatology was the result that Dusser de Barenne and Sager obtained when a small amount of strychnine was injected into the hypothalamus. The animal, after emerging from the anesthetic, rushed madly through the room, jumped up against

the walls, miaowed continually, showed signs of being furious, and spat on the slightest sign of being approached. There was polypnea which continued even after the cat had become quiet. This intense activity in the skeletal musculature was accompanied by a general hypersensitivity of the skin of the entire body and by signs of marked activity in the autonomic system (maximally dilated pupils, erection of hair of back and tail, salivation, defecation and urination). It is significant that this behavior bears a considerable resemblance to the sham rage.

The Rage Responses of Decerebrate Preparations and their Relationship to Decorticate Sham Rage.—The failure of the decorticate sham rage to develop in his acute mesencephalic preparations was not the only evidence that Bard brought forward in support of the view that central mechanisms located below the diencephalon cannot account for that condition. Reference was made to the fact that nowhere in the numerous reports which have been given of the behavior of decerebrate [3] cats in the acute condition is there a description of any behavior approaching the activities of the typical sham rage. It was pointed out that Woodworth and Sherrington (39) had obtained certain responses, expressive of affective states, in acute experiments on decerebrate cats. But these 'pseudaffective reflexes,' as they were called, were only obtained on central stimulation of a large nerve trunk. Although they had a certain 'width of coördination,' Woodworth and Sherrington pointed out that "they never amounted to an effective action of attack or escape." The sham rage stands in sharp contrast to these reflexes, for it is elicited by far less severe stimuli and it possesses a far greater breadth and energy of expression. This statement appeared in essentially the same form in the original report by Bard (1), but it is now made with even greater assurance on the basis of experiments in which, for various purp ses, the writer and

[3] Following Sherrington and in accord with the proper definition of the word *cerebrum*, physiologists and neurologists generally use the adjective *decerebrate* to designate the condition which ensues upon mesencephalic transection of the brain stem. *Decerebration* and *decortication* are quite different procedures and should not be confused.

his collaborators have repeated the procedures carried out by Woodworth and Sherrington.

In physiological work it is sometimes difficult to evaluate negative results and this is especially the case when they are obtained in acute experiments. But in the experimental delimitation of the central source of the decorticate sham rage the negative results obtained after mesencephalic transections were of distinct value. The eleven animals subjected to that procedure survived for periods ranging from 4 to 19 hours. In every case the typical activity failed to develop, whereas it appeared within 35 minutes of the completion of the operation in all but one of the positive experiments. Although the difference in behavior of the two groups was most impressive, it was correlated, anatomically, with a difference in level of transection of not more than two or three millimeters. This combination of facts lends to the negative results a significance which cannot be over-emphasized.

Ablations of central nervous tissue are frequently followed by phenomena which are the immediate and temporary results of the surgical intervention. They can be attributed to a depressive effect of the insult, in other words to 'shock,' or to an irritative effect of some kind. It is important to distinguish between these transient results and the permanent changes which are produced. As Hughlings Jackson first pointed out many years ago, the latter fall into two distinct groups, deficiency phenomena and release phenomena. It is worth considering whether the absence of easily evokable rage reactions in the decerebrate cats of acute experiments is not due to some sort of shock. It was the recognition of this possibility that led Bard (1) to make pointed reference to the chronically decerebrate cats of Bazett and Penfield (5). After low mesencephalic truncation of the brain stem some of these animals lived for periods of time sufficient to assure freedom from shock and the irritative effects of the operation. Although Bazett and Penfield noted tail lashing, kicking, running, even biting, and, very occasionally, vocalization, the sum total of pseudaffective responses occurring in any one individual obviously fell far short of the *intense, prolonged,*

and *widespread* motor activity which constitutes the sham rage of the acutely decorticate cat.

Recently Keller (27) has reported the occurrence of rage responses in cats which he succeeded in maintaining for as long as 18 and 20 days after decerebration. The specific reactions were somewhat more readily elicited after cutting through the middle and lower portions of the midbrain than after high mesencephalic transections. This 'rage' was evidenced by 'hissing, growling, wagging of the tail, pawing movements and erection of the hair.' There were also indications of general sympathetic discharge. Signs of rage were provoked not only by the sort of stimulation that is effective in normal cats but also by such slight disturbances as lifting the preparations. Absent was "the purposeful somatic element of 'escape' and 'defense.'" The conclusion that "the discharging of a typical rage response is not dependent on the brain stem cephalad to the middle level of the midbrain" seems justified by the evidence which is presented. Keller, however, states that these activities differ from the sham rage of my preparations only in not occurring spontaneously and in persisting only as long as stimulation continues. In my opinion the difference is greater than that. The 'typical rage response' which occurred in Keller's experiments never approximated, either in magnitude or intensity, the sham rage of acute decorticate and hypothalamic preparations. The protocols published by Keller give no indication that his animals ever exhibited at any one time more than a few of the items of behavior which constitute the full expression of rage seen in acute decorticate cats or in the infuriated normal animal. In other words the 'typical rage response' alluded to was a definitely sub-maximal response. It appears that the full capacity of expression is not present unless the hypothalamic region is intact.

In comparing the results obtained by Bazett and Penfield and by Keller, on the one hand, with those reported by Cannon and Britton and by Bard, on the other, recognition should be given the fact that the experimental conditions were not the same. The former group of investigators induced the

rage responses by such manipulations as handling, pinching the skin and passing a stomach tube. The latter group studied the sham rage in cats which were tied in the dorsal position on an animal board, a situation which in itself constitutes, for a normal cat at least, a strong rage stimulus. Nevertheless, there is a fair basis for comparison. The activities appearing under the condition of restraint in hypothalamic cats are usually intermittent—fits of sham rage lasting from a few seconds to several minutes alternate with intervals of nearly complete quiescence. Under the circumstances, the truly amazing activity does develop 'spontaneously' without the application of any external stimulus, although, during a quiet period, any slight disturbance may bring on the display of rage. Sometimes a fit of activity persists for as long as ten or twelve minutes. When the animals are released from the thongs that secure the feet and are placed on their sides they usually become quiet, but any slight disturbance will throw them into such vigorous paroxysms that the experimenter must exert considerable care in handling them lest he be injured by teeth or claws. There is no evidence that any activity as intense or so thoroughly suggestive of fury can be induced in a chronic midbrain or bulbo-spinal cat.

Finally, the very fact that there is such a tremendous contrast in behavior between hypothalamic and decerebrate cats in the acute condition is sufficient evidence that the diencephalic mechanism adds a prepotent factor for the expression of rage. Even were it argued that the inactivity of acute decerebrate preparations is due to some sort of 'shock' there remains the fact that such 'shock' is absent in hypothalamic animals. Thus there seems to be no escape from the conclusion that the caudal hypothalamus contains a neural mechanism which has much to do with the vigorous expression of rage. The whole matter can still be summed up by a statement made five years ago (1, page 509): "Certain elements of affective behavior may even be induced in the spinal cat, and still more in the bulbo-spinal or midbrain preparations, but the results of this investigation indicate that it is

only when the diencephalic mechanisms are present that these elements can be readily welded together to form the rage reaction." It may be added that by rage reaction was meant, of course, the decorticate sham rage.

The Nature of Sham Rage.—The term 'sham rage' was introduced by Cannon and Britton (**10**). It was adopted by Bard (**1, 2, 3**), although in common with Cannon he has used the expressions 'quasi-emotional' and 'pseudaffective' in referring to the emotional display which is under discussion. Since, as is soon to be related, decorticate animals are capable of other forms of emotional expression and since this particular type of activity closely resembles that of the enraged normal animal, 'sham rage' is perhaps the more correct designation. And now a word about the adjective 'sham' and the prefixes 'quasi-' and 'pseudo-.' The chief reason for their use has been to indicate the obvious. That various animals have subjective experiences is a supposition that is difficult to deny. Certainly a good many psychologists take it for granted, and with those of us who perform experiments on animals as well as with the antivivisectionists this is an assumption that is acted upon with conviction. It seems reasonable to believe that after removal of the higher parts of the brain, including the cerebral cortex, there is an absence, or at least a profound modification, of the conscious aspects of emotion. The term emotion implies two things: a way of acting and a subjective experience. Therefore, in the case of a decorticate cat it would be confusing to speak unqualifiedly of its emotion. One may, however, speak with assurance of its *expression* of emotion. The activity under discussion is termed rage because it unmistakably 'simulates the expression of anger seen in the normal cat' (**1**); it is called sham because we cannot, so far as may be judged by clinical experience, suppose that with cortex removed, consciousness, including the subjective aspects of rage, is unchanged or even present.[4]

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[4] Since one of the definitions of both 'sham' and 'pseudo-' implies something false or counterfeit, while 'quasi-' means 'as if,' or 'having some resemblance,' it might have been more accurate to use the more awkward expression 'quasi-rage.'

So obvious are these ideas and reasons that it seems a pity to cover printed space with them. But in writing on the neurological aspects of emotion experience teaches that it is necessary to present facts and ideas carefully and in elementary fashion lest they be recast in some scarcely recognizable form. An example of the sort of juggling that the facts may undergo has already been indicated in the remarks on the use to which Harlow and Stagner have put my experimental results. Those authors exhibit their proficiency at this sort of thing in an even more flagrant manner when they state (**18**, page 585) that "Both Bard and Cannon consistently refer to these conditions as emotional (the thalamus as the 'seat of the emotions,' etc.) and Cannon especially misuses the term 'emotion' in referring to feeling states." The 'conditions' referred to are the activities of the sham rage. The assertion that either Cannon or Bard has ever referred to the thalamus as 'the seat of the emotions' is not only quite without foundation but is contrary to the expressed views of both. When Newman, Perkins and Wheeler (**29**), with the same lack of understanding as has been displayed by Harlow and Stagner, ascribed to Cannon the view that the thalamus is the 'seat of the emotions,' a strong protest was forthcoming (**9**, page 291). And it is important to note that Cannon has not stated anywhere that "the thalamus is the seat of affectively toned experiences" (see **18**, page 286). As for Bard, he has employed the expression 'seat of the emotions' just twice and never in connection with the thalamus. This use occurred first in the introductory sentence of his first paper (**1**) and was as follows: "At various times 'the seat of the emotions' and the central mechanisms responsible for emotional behavior have been sought in the cerebral cortex." His second and only other use of the expression was in another paper (**3**, page 469), where the same statement was repeated. It is obvious that this is a historical reference only and the placing of quotation marks around the phrase indicated something other than addiction to the ideas it implies.

Apparently Harlow and Stagner do not understand that what Cannon and Bard have been talking about in referring

to decorticate animals is not emotion as such, but the expression of emotion. Repeatedly and with consistency Bard has used in his reports on sham rage such expressions as 'the capacity for the exhibition of anger,' 'the nervous organization for the display of rage,' 'the subcortical management of several types of emotional expression.' He has not been concerned with the subjective aspects of emotion except to point out (3) that certain clinical material, in which evidence on this aspect of emotion can be obtained, is of importance in considering the validity of the James-Lange theory, and he has, of course, taken it into consideration in describing Cannon's diencephalic theory. From first to last he has carefully distinguished between the conscious aspects of emotion and the expression of emotion. And in this he has followed Cannon. Indeed, the whole essence of Cannon's theory of emotion lies in a clean-cut differentiation of the neural processes which lead to emotional consciousness and those which result in emotional expression. This should have been clear enough to Harlow and Stagner, for their own theory, as they describe and schematize it, appears as nothing whatever but Cannon's theory modified by the hypothesis that 'four fundamental feelings' have their anatomical 'seat' in the thalamus and make 'emotion' when a 'cognitive experience' of cortical origin is added to them.

Harlow and Stagner, in maintaining that the activities of the sham rage represent 'excitement' and not the expression of rage, write (18, page 584): "Although Bard calls the state of emotional excitement 'sham rage,' the essential characteristic of his preparation is a state of increased bodily activity dominated by violent sympathetic discharge, and it is fully as apt to characterize the behavior of this animal as representing excitement (feeling of excitement, if conscious) as to ascribe to the animal a particularized emotion of rage." Of course Bard did not call it rage; he called it sham rage, which, as already explained, describes an expression of rage. Next they remark: "Removal of cortical control has apparently left the animal in a state of chronic excitement (which we feel is more aptly described as a feeling than as an emotion since the latter

is characteristically induced by some external animal or object). . . ." So concerned are Harlow and Stagner with their idea of a 'feeling of excitement' whose anatomical seat is in the thalamus, that they are willing to imply that this feeling can be sustained by a cat with no cortex and with brain stem transected in such a way that scarcely any thalamus remains!

The description of sham rage as a characteristic *expression* of rage was based on considerable experience with cats. This experience leads me to take sharp issue with Harlow and Stagner when, in appraising the sham rage, they remark, "the greater number of these responses would characterize the emotion of 'fear' as adequately as they would the emotion of 'rage,' and in view of the work of Landis in differentiating emotional responses in the human being, it is more than doubtful if naïve observers would more frequently identify the behavior of these cats as exhibiting rage than fear." The reference to Landis' observations on *human beings*, whatever their adequacy may be, is irrelevant. But it is not difficult to distinguish between an expression of rage and an expression of fear *in the cat*. It is doubtless impossible to determine whether a cat standing up to an attacking or threatening dog is *subjectively* experiencing rage or fear or a mixture of both, but on the supposition that different things should have different names it can be asserted that a normal cat is capable of *displaying* both rage and fear. When as a result of painful or rough treatment or in response to attack by another animal a cat reacts by spitting and aggressive biting and clawing, it is proper to call it one thing. When in response to similar stimuli or to other happenings, especially a sudden and unexpected loud noise, this same cat dashes off in a furtive or precipitate manner, mewing plaintively, and tremblingly goes to cover on the first opportunity, it is necessary to designate this as something else. General usage leads one to call the former an expression of anger, the latter a display of fear or terror.

In writing that Bard, in his summary, "goes only so far as to state that the decorticate behavior 'resembles' rage,"

Harlow and Stagner definitely misquote him, for what he actually wrote was, "This behavior simulates the expression of anger as seen in the normal cat and is best described as sham rage." Furthermore, in his discussion Bard stated, "Anyone who has ever tied an unruly cat to an animal board will agree that the sham rage shown in these experiments closely resembles the behavior of the infuriated normal animal." Here is the essence of the whole matter. In case Harlow and Stagner ever take the trouble to study at first hand sham rage in the decorticate cat or to tie a vigorous normal cat to an animal board, they should be given this warning: the animal will, if given the opportunity, bite and scratch them; it will not cringe, tremble, mew plaintively or show other evidence of fear. Should they go so far as to prepare and thoroughly study decorticate cats in the chronic condition they will find, as Bard has found (see Part II), that these preparations can *display* fear, rage, and, at least in the case of the female, sexual excitement.

In view of these facts, it is to be hoped that not only Harlow and Stagner but other psychologists who are of similar mind will realize that emotions, as patterns of response, do exist and that there is more support for this statement than 'the force of tradition and an incapacity for looking beyond words.' Perhaps these observations on decorticate cats will raise in the minds of Harlow and Stagner some doubt as to the validity of that aspect of their theory of emotions, which supposes that the specificity of an emotion is dependent upon cognitive processes whose anatomical seat is in the cerebral cortex. Certainly the observations which have been made on the *expressions* of emotion in decorticate animals do not support the conclusion (19, page 194) that "Differentiation of specific emotions is to be considered not as a matter of different motor components, but simply as differences in the conscious attitude toward the stimulation." In this connection it should be understood that I make no denial of the suggestion that *subjectively* emotions are to be 'distinguished from feelings in that emotion is characterized by cognition of the external situation.'

The Relation of Cortex and Thalamus to Feelings.—It must not be supposed that the present writer is in total disagreement with all the views of Harlow and Stagner on feelings and emotions. For example, he finds most plausible their treatment of pain as a feeling which takes on the aspects of a sensation only when other more discriminative elements of consciousness are simultaneously aroused. Their abundant use of the incomparable clinical studies of Head and Holmes (20) can only be objected to on the grounds that these studies have been presented as something more than good circumstantial evidence that the thalamus is the seat of feelings of pain and pleasure. Important and suggestive as that work is, it does not prove that the cortex is unconcerned in the matter of these items of consciousness. It is well to realize that in reporting on their affective experiences patients afflicted with the *syndrome thalamique* make use of cognition and motor activities (speech) of cortical origin. Therefore it is somewhat difficult to exclude their cortices from those parts of the brain which serve as the anatomical basis of their feelings. This point would seem to have a special application in those cases of the *syndrome thalamique* in which an emotional stimulus gives rise to highly unpleasant sensations and feelings in the affected side of the body. For example, one patient, a woman (Case 11 of Head and Holmes), had always been musical and up to the time of her stroke enjoyed good music intensely, deriving from it a most satisfactory emotional response; but after her attack that type of music produced such uncomfortable sensations (feelings) in the right half of the body that she was obliged to get away from the environmental cause of her discomfort. Other types of music such as popular songs left her cold, and indifferent sounds, such as the note of a tuning-fork or the sound of a bell, produced no abnormal effect. Here complicated past experiences involving cognitive processes and conditioning at the cortical level were prime factors in establishing the over-activity of the feeling-tone after the *syndrome thalamique* had developed. Does this make it permissible to rule out the cortex as a participant in the development of the unpleasant feeling?

The nature of the *syndrome thalamique*, the absence of disturbances in pain after stationary cortical lesions and the failure to produce pain by direct cortical stimulation in conscious patients are facts which have been put forward to show that only subcortical structures, particularly the thalamus, serve as the anatomical substrata for pain or (to use Harlow and Stagner's expression) the feeling of pain-unpleasantness. But other facts suggest that the cortex cannot be so easily ruled out. It is to be remembered that in cat and monkey Dusser de Barenne (11, 13) was able to induce signs of hyperalgesia by application of strychnine to a small area anywhere within a large cortical sensory zone. His results on monkeys showed that the sensory cortex of this primate is more extensive than had been supposed, for it was found to overlap both the precentral and the postcentral areas. After local application of strychnine to the sensory zone of one side, evidence of hyperalgesia of the skin could be found on both sides of the body, and this even occurred after bilateral ablations of all parts of the sensory zones except a fraction surrounding the strychninized area. If it should be shown that in man the cortex is actually concerned with a feeling of pain, these observations suggest that it might require extensive bilateral lesions, a type of injury not often encountered in the clinic, to produce on one side of the body a disturbance in the sensitivity to pain. On the other hand Dusser de Barenne and Sager (14) have found that in cats the symptomatology (signs of paresthesiæ, hyperesthesia and hyperalgesia) of localized thalamic strychninization is practically identical with that given by cortical application of the drug, and further, that after total removal of both cerebral cortices certain typical effects of the thalamic application could be detected. This might be construed as favorable to the idea that the thalamus alone is concerned with pain, but a symptom, however expressed, cannot be taken as proof of conscious experience. The negative results of direct electrical stimulation of the human cortex are not surprising, when one considers the histological complexity of the tissue. Since electrical stimulation of the motor cortex is incapable of evoking postural reac-

tions which are dependent upon it (4), there is no reason to suppose that similar stimulation of sensory cortex should induce every sensation subserved by it. While these considerations lead to no definite conclusion concerning the question whether the cortex plays a rôle in the feeling of pain, they do make an assertion that the cortex plays no such rôle definitely hazardous.

BIBLIOGRAPHY

1. BARD, P., A diencephalic mechanism for the expression of rage, with special reference to the sympathetic nervous system, *Amer. J. Physiol.*, 1928, **84**, 490–515.
2. BARD, P., The central representation of the sympathetic system as indicated by certain physiologic observations, *Arch. Neur. & Psychiat.*, 1929, **22**, 230–246.
3. BARD, P., The neuro-humoral basis of emotional reactions (In Foundations of experimental psychology, Worcester, 1929).
4. BARD, P., Studies on the cerebral cortex: I. Localized control of placing and hopping reactions in the cat and their normal management by small cortical remnants, *Arch. Neur. & Psychiat.*, 1933, **30**, 40–74.
5. BAZETT, H. C., AND PENFIELD, W. G., A study of the Sherrington decerebrate animal in the chronic as well as the acute condition, *Brain*, 1922, **45**, 185–265.
6. BEATTIE, J., BROW, G. R., AND LONG, C. N. H., Physiological and anatomical evidence for the existence of nerve tracts connecting the hypothalamus with spinal sympathetic centres, *Proc. Roy. Soc. Lond., B*, 1930, **106**, 253–275.
7. CANNON, W. B., Bodily changes in pain, hunger, fear and rage, 2nd ed., New York, 1929.
8. CANNON, W. B., The James-Lange theory of emotions; a critical examination and an alternative theory, *Amer. J. Psychol.*, 1927, **39**, 106–124.
9. CANNON, W. B., Again the James-Lange and the thalamic theories of emotion, PSYCHOL. REV., 1931, **38**, 281–295.
10. CANNON, W. B., AND BRITTON, S. W., Studies on the conditions of activity in endocrine glands: XV. Pseudaffective medulliadrenal secretion, *Amer. J. Physiol.*, 1925, **72**, 283–294.
11. DUSSER DE BARENNE, J. G., Experimental researches on sensory localizations in the cerebral cortex, *Quart. J. Exper. Physiol.*, 1915, **9**, 355–390.
12. DUSSER DE BARENNE, J. G., Recherches expérimentales sur les fonctions du système nerveux central, faites en particulier sur deux chats dont le néopallium avait été enlevé, *Arch. néerl. de physiol.*, 1919, **4**, 31–123.
13. DUSSER DE BARENNE, J. G., Experimental researches on sensory localization in the cerebral cortex of the monkey (Macacus), *Proc. Roy. Soc. Lond., B.*, 1924, **96**, 272–291.
14. DUSSER DE BARENNE, J. G., AND SAGER, O., Über die sensiblen Funktionen des Thalamus opticus der Katze, Untersucht mit der Methode der örtlichen Strychninvergiftung; allgemeine Symptomatologie und funktionelle Lokalisation, *Zsch. f. d. ges. Neur. u. Psychiat.*, 1931, **133**, 231–272.
15. FULTON, J. F., New horizons in physiology and medicine: the hypothalamus and visceral mechanisms, *New Eng. J. Med.*, 1932, **207**, 60–68.

16. FULTON, J. F., AND INGRAHAM, F. D., Emotional disturbances following experimental lesions of the base of the brain (pre-chiasmal), *J. Physiol.*, 1929, **67**, *Proc. Physiol. Soc.*, p. xxvii.

17. GOLTZ, F., Der Hund ohne Grosshirn, *Pflüg. Arch. f. d. ges. Physiol.*, 1892, **51**, 570–614.

18. HARLOW, H. F., AND STAGNER, R., Psychology of feelings and emotions: I. Theory of feelings, PSYCHOL. REV., 1932, **39**, 570–589.

19. HARLOW, H. F., AND STAGNER, R., Psychology of feelings and emotions: II. Theory of emotions, PSYCHOL. REV., 1933, **40**, 184–195.

20. HEAD, H., AND HOLMES, G., Sensory disturbances from cerebral lesions, *Brain*, 1911, **34**, 102–254.

21. HIMWICH, H. E., AND KELLER, A. D., Effect of stimulation of hypothalamus on blood glucose, *Amer. J. Physiol.*, 1930, **93**, 658.

22. HOUSSAY, B. A., AND MOLINELLI, E. A., Centre adrénalino-sécréteur hypothalamique, *C. r. Soc. de Biol.*, 1925, **93**, 1454–1455.

23. INGRAM, W. R., HANNETT, R. I., AND RANSON, S. W., The topography of the nuclei of the diencephalon of the cat, *J. Comp. Neur.*, 1932, **55**, 333–394.

24. ISENSCHMID, R., AND KREHL, L., Über den Einfluss des Gehirns auf die Wärmeregulation, *Arch. f. exper. Path. u. Pharmakol.*, 1912, **70**, 109–134.

25. ISENSCHMID, R., AND SCHNITZLER, W., Beitrag zur Lokalisation des der Wärmeregulation vorstehenden Zentralapparatus im Zwischenhirn, *Arch. f. exper. Path. u. Pharmakol.*, 1914, **76**, 202–223.

26. KARPLUS, J. P., AND KREIDL, A., Gehirn und Sympathicus, *Pflüg. Arch. f. d. ges. Physiol.*, 1909, **129**, 138–144; 1910, **135**, 401–416; 1911, **143**, 109–127; 1918, **171**, 192–200; 1927, **215**, 667–670.

27. KELLER, A. D., Autonomic discharges elicited by physiological stimuli in midbrain preparations, *Amer. J. Physiol.*, 1932, **100**, 576–586.

28. KELLER, A. D., AND HARE, W. K., The hypothalamus and heat regulation, *Proc. Soc. Exper. Biol. and Med.*, 1932, **29**, 1069–1070.

29. NEWMAN, E. B., PERKINS, F. T., AND WHEELER, R. H., Cannon's theory of emotion: a critique, PSYCHOL. REV., 1930, **37**, 305–326.

30. RANSON, S. W., AND MAGOUN, H. W., Respiratory and pupillary reactions induced by electrical stimulation of the hypothalamus, *Arch. Neur. and Psychiat.*, 1933, **29**, 1179–1194.

31. RIOCH, D. McK., Studies on the diencephalon of carnivora: I. The nuclear configuration of the thalamus, epithalamus and hypothalamus of the dog and cat, *J. Comp. Neur.*, 1929, **49**, 1–119.

32. RIOCH, D. McK., Studies on the diencephalon of carnivora: II. Certain nuclear configurations and fiber connections of the subthalamus and midbrain of the dog and cat, *J. Comp. Neur.*, 1929, **49**, 121–153.

33. RIOCH, D. McK., Studies on the diencephalon of carnivora: III. Certain myelinated fiber connections of the diencephalon of the dog, cat, and aevisa, *J. Comp. Neur.*, 1931, **53**, 319–388.

34. ROTHMANN, H., Zusammenfassender Bericht über den Rothmannschen grosshirnlosen Hund nach klinischer und anatomischer Untersuchungen, *Zsch. f. d. ges. Neur. u. Psychiat.*, 1923, **87**, 247–313.

35. SACHS, E., On the relation of the optic thalamus to respiration, circulation, temperature and the spleen, *J. Exper. Med.*, 1911, **14**, 408–432.

36. SCHALTENBRAND, G., AND COBB, S., Clinical and anatomical studies on two cats without neocortex, *Brain*, 1931, **53**, 449–491.

37. SPIEGEL, E. A., Die Zentren des Autonomen Nervensystems, Berlin, 1928.
38. WINKLER, C., AND POTTER, A., An anatomical guide to experimental researches on the cat's brain, Amsterdam, 1914.
39. WOODWORTH, R. S., AND SHERRINGTON, C. S., A pseudaffective reflex and its spinal path, *J. Physiol.*, 1904, 31, 234–243.

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MASSED AND DISTRIBUTED PRACTICE IN PUZZLE SOLVING

BY T. W. COOK

Acadia University

The current tendency of research workers in psychology to treat any given aspect of the learning process in its dynamic relationships, *i.e.*, as a function of factors that condition its operation, is the theme of Ruch's discussion of "Factors affecting the relative economy of massed and distributed practice in learning." [1] Ruch finds the following of major importance: "first, the general characteristics of the distribution of practice (number and length of periods, interval between periods, degree of learning being considered, etc.); second, the type of material being learned; third, the age of the subjects; fourth, criterion or aim of the learning (immediate or delayed recall, speed, accuracy, and amount of recall, improvement, etc.); fifth, the order of repetitions within a practice period (whole *vs.* part order); sixth, manner of studying; seventh, the stage of learning (whether the distribution is equally effective at the initial and final stages of learning and in the exercise of a well-learned habit)." Valuable as are such analyses, progress comes not by uncritical acceptance but by constant revaluation in the light of experimental data. Are the assumed variables really distinct, or are they merely different names for the same process? There is also the third possibility that important differences are concealed under identical verbal tags. One method of approach to a more fundamental classification of the factors conditioning the efficiency of massed and distributed procedure consists in relating such factors to the total pattern of the learning process. It is probable that similarities and differences found to be significant in the wider field can be used to evaluate conditions affecting the more restricted problem of distribution of practice,

[1] T. C. Ruch, PSYCHOL. REV., 1928, 35, 19-45.

and, in addition, it is conceivable that a properly directed study of the latter might yield data significant for controversial points of general theory.

An inquiry so begun calls for an examination of prevalent theories of the nature of the learning process. Until the beginning of the present century the majority of theorists (except the 'association school'), as well as 'common sense,' recognized a more or less explicit duality. Intelligent learning and habit formation were regarded as having little or nothing in common. During recent years a radical change has taken place. Thorndike, Pavlov, and the Behaviorists have forsaken dualism and agree in postulating a monistic theory of learning. For all three, however, the habit aspect is basic. All learning is connection-forming, and the only ultimate distinctions lie in the number or strength of 'bonds.'

On the other hand Tolman, a psychologist self-classed as a behaviorist but in this regard more akin to the English Purposivists or the Gestalt school, sponsors a view which makes learning wholly a matter of intelligent adjustment.

Yet dualism still finds defenders. McDougall, following Stout and the English purposive tradition, distinguishes between intelligent learning, which consists in the discovery of significant similarities and differences, and habit formation, which he conceives to be a purely 'mechanical' process. In his discussion of reproduction he adopts Bergson's separation of memory from habit, memory being the revival of intellectual dispositions built up by apperception of similarities and discrimination of differences, and habit the reactivation of mental structure formed by mechanical repetition.[2] Gestalt psychologists have evolved a characteristic reinterpretation of the old dichotomy. While a full consideration of the subtle similarities and differences between the theoretical positions of Tolman, McDougall, and the Gestalt school, is outside the scope of this paper,[3] two features of Gestalt interpretation are

[2] For the most recent exposition of McDougall's view, see The energies of men, New York, 1933.

[3] R. H. Wheeler, F. T. Perkins, and S. H. Bentley (PSYCHOL. REV., 1933, 40, 423-429) discuss the question from the Gestalt standpoint. Their otherwise excellent treatment is, I think, vitiated to some extent by failure to fully understand McDougall's theory.

relevant: (1) Though avowedly monistic, the intelligence-habit distinction is at least superficially apparent in much Gestalt theorizing. Koffka separates 'achievement,' or the formation of a configuration (which may be a relatively sudden process), from 'memory,' or the conditions that determine the reinstatement of a configuration.[4] Köhler in his experiments with apes was primarily interested in 'insight,' but speaks of 'crude stupidities arising from habit'[5] and 'the reproduction of a mechanical proceeding.'[6]   (2) The intellectual rather than the mechanical view of learning is stressed by Gestalt theorists. This may be due in part to a desire to call attention to aspects neglected by the prevalent associationism, but the fact is patent. The nature, manifestations, and factors determining 'insight' form the major part of Gestalt discussions on learning.

To the unbiased observer the postulation of a rigid monism in such a chaotic field as learning seems premature, and not very likely to lead to useful hypotheses. It appears better to follow obvious lines of cleavage in the data. Since one of the most clear-cut of these is the distinction between intelligent learning and rote learning, it might be advisable to classify the material already gathered and carry out a program of experimentation designed to discover whether the distinction is superficial or fundamental. As a starting point we may summarize the dualistic interpretation. Learning has two aspects, the intellectual and the mechanical. Intelligent learning consists in the appearance and clarification of insight (Köhler and Koffka), or the discovery of relationships (McDougall, Spearman). Mechanization is fixing or setting of material already understood. The view of course implies that mind is an organization and intelligent learning a matter of reorganization rather than addition.[7]

[4] K. Koffka, The growth of the mind, New York, 1928, p. 169.

[5] W. Köhler, The mentality of apes, New York, 1926, p. 154.

[6] Ibid., p. 250.   Among other experimental psychologists inclining to dualism are Dodge (Protopraxic and epicritic stratification of human adjustments, Washburn Commemorative Volume, Ithaca, 1927, 145–157); and Maier (Reasoning in white rats, Comp. Psychol. Monog., 1929, 6, no. 3). Spearman's 'two-factor' theory is also well-known.

[7] But it does not imply vitalism, materialism, idealism, or any other philosophical theory.

In attempting to find the bearing of massing or distributing practice on the truth or falsity of the dualistic standpoint, a consideration of the unit of practice is of first importance. Two units have been used: (*a*) a constant time unit, and (*b*) a 'trial' or repetition of the stimulating situation. In studies of rote learning in which nonsense syllables or digits are exposed for a constant time and with a uniform interval between presentations, the amount of time spent in practice and the number of repetitions are proportional to one another. But in experiments on maze-learning or mirror-tracing where a trial means one successful performance, the amount of time spent in practice is less (on the average) in each successive trial. The latter consideration has led to a feeling in certain quarters that a time unit is more exact and that experiments on problems such as maze-learning are exposed to a special, though perhaps unavoidable, source of error. Without questioning the practical value of a study of the most economical amount of time for practice in any given function, investigation of the theoretical question under consideration requires the use of a trial or repetition of the stimulating situation as the unit of practice. The value of a comparison of massed and distributed procedure as a tool of psychological analysis lies in the possibility it admits of tracing the development of a given function. Since novel insight *is* intelligent learning, the place at which intelligent grasp of the total situation occurs is the end of a unit, and it is at this point that a time interval must be inserted if it is to have any value in differentiating the effect of massed and distributed practice on intelligent and rote learning. It is of course true that the precise time at which a novel insight appears is not always easy to determine. In problems where the goal can only be reached by obtaining some comprehension of the total pattern of the situation, such comprehension may be assumed as soon as and only when the goal is reached. But in tasks such as tracing a maze, where random movements necessarily play a large part and the 'end' can be attained by chance, it is extremely difficult to decide whether a successful run means any genuine advance in understanding the problem. The

most one can say with assurance is that if a novel insight does occur, it will in all probability be at the end of a trial.

When a repetition of the stimulating situation is taken as the unit of practice, it is evident that in many investigations the so-called 'distributed' procedure in reality contains an indeterminate amount of massing. Thus Lorge's experiments on memorizing number materials and on substitution[8] had one minute of working time as the unit of practice in distributed procedure. Since there were only twelve pairs of numbers on a page, one minute of practice involved some massing of repetitions of the same material in all practice periods.

From the 'two-type' theory of learning certain a-priori deductions may be made which are capable of being tested by data already at hand or by future experiments:

1. On the whole, intelligent readjustment should be favored by massed practice and the 'fixing' or 'setting' process by distributed practice. This is more or less vaguely recognized by Woodworth's statement that "when a fact has been observed it can immediately be put to use," [9] and by Lorge, who says that "a distinction must be made between the advantageous effects of time and the deleterious effects of time." [10] Neither author seems to see the full significance of his words, because both fail to recognize that the appearance of a novel configuration is a distinct form of learning. The assumption of the present writer is that (with certain limitations specified later) greater economy in learning is obtained by massing practice immediately after the appearance of a configuration. The new pattern is thus clarified and stabilized and a lengthy process of reaching a solution by "approximation and correction" [11] need not be reverted to on subsequent trials. The retentivity of a clear and stable configuration, on the other hand, is favored by distributed practice.

[8] I. Lorge, Influence of regularly interpolated time intervals upon subsequent learning, *Teachers College, Columbia Univ. Contrib. Educ.*, no. 438, New York, 1930, 23-31.

[9] R. S. Woodworth, Psychology, New York, 1929, p. 87.

[10] I. Lorge, *op. cit.*, p. 29.

[11] Dodge's phrase is more accurately descriptive than 'trial and error.'

2. It follows that the relative efficiency of massed versus distributed practice in intelligent learning is a function of the stability of the novel configuration. If a pattern is highly integrated when it first appears, repetition might with economy be postponed for days or even weeks. A highly unstable configuration, on the contrary, would require one or more repetitions immediately after its formation. An example of a configuration having a high degree of tenacity upon its first appearance is the current cross-word puzzle poser 'a four letter word ending in eny.' If writing is not permitted a solution is often most elusive, but once found will be reproducible without intervening practice after weeks, months, or even years. Instances of novel and unstable configurations are given by many familiar puzzles. The best examples in the experimental literature, however, come from Köhler's investigations of apes. Köhler set his animals a series of tasks graded in increasing order of difficulty, until he reached the limits of the apes' unaided grasp of the functions of simple implements. All his experiments were designed as intelligence tests, and were arranged to be practically insoluble by random manipulation without insight. In all cases where the difficulty of the problem approached the limit of an animal's intelligence, the novel configuration whose appearance *was* the solution of the problem seemed relatively unstable. Although Köhler was not concerned with the effect of massing or distributing repetitions, there are indications in his data that such newly-formed configurations gained greatly in stability if one or more repetitions were taken at once. The apes usually solved the problem without hesitation in an immediate second trial. Köhler states that "it is always a sign of extreme difficulty if a rapid repetition of the test does not result in a rapid repetition of the solution just found." [12]

3. The learning of meaningful material should (in general) be facilitated by massed practice and the learning of nonsense material by distributed practice. The experimental data, though scanty, afford at least a partial support to this con-

[12] W. Köhler, *op. cit.*, p. 175.

clusion. Lyon [13] compared no interval between presentations with one presentation per day for prose, poetry, nonsense syllables, and digits, with a wide variation in amount of material, and found that the distributed procedure was much more economical with nonsense materials, while with poetry and prose there was no advantage for either massing or distribution. Lyon's tests of efficiency of learning were taken at the end of each period of practice. The results of Austin with meaningful material are similar. Austin [14] found that, for recall after 24 hours, 1 reading a day for 5 days was equal in efficiency to 5 readings in immediate succession. Some class experiments of Gordon yield parallel data.[15] For immediate recall, 3 massed presentations of logical prose were more effective than 1 presentation a week during 3 weeks. On the other hand, experiments by Bergstrom, Ebbinghaus, Jost, Piéron, Mould, Treadwell and Washburn, and Lorge with nonsense materials uniformly favor distribution of practice.[16] The problem is so complicated by other factors, particularly immediate versus delayed recall, that further consideration of experimental results will be postponed until some of these factors have been examined.

It should be noted that from the standpoint here adopted the phrase meaningful _material_ is ambiguous. The meaningfulness lies not in the material, but in the mental organization of the subject. It is not the physical stimulus that is more or less meaningful, but the psychic response. A page of Chinese is (relatively) meaningless to me and highly meaningful to a literate Chinaman. How, then, does a passage become meaningful? We have previously rejected the view that meaningfulness is merely a matter of 'widely ramifying associations,' and adopted the position that the explanation lies in mental organization into systems of relationships. The learning of a prose passage is thus primarily a matter of

[13] D. O. Lyon, The relation of length of material to the time taken for learning and the optimum distribution of time, _J. Educ. Psychol._, 1914, 5, 115–163.

[14] S. D. M. Austin, A study in logical memory, _Amer. J. Psychol._, 1921, 32, 370–403.

[15] K. Gordon, Class results with spaced and unspaced memorizing, _J. Exper. Psychol._, 1925, 8, 337–343.

[16] See Ruch's paper (footnote 1) for summaries and bibliography.

reorganizing one's mental pattern into a system of relationships relevant to the author's thought. The significant distinction (for our purposes) is between meaningful and non-meaningful *learning*, rather than between meaningful and non-meaningful material. The learning process is of the first type as long as the subject is progressing toward an understanding of the passage or problem, it is of the second type as soon as he has grasped the significance and the meaning no longer changes during the period of experimentation. It follows that experiments with nonsense materials can give little information concerning the 'insight' phase of learning. This fact is not incompatible with the claim of Gestalt psychologists that even learning nonsense syllables involves the formation of a configuration. It does imply, however, that the amount of change in mental organization during the period of experimentation is so slight that this type of learning remains sharply marked off from the rapid progress in understanding that takes place in solving puzzles or reading difficult prose.

The methods of Austin, Gordon, and Lyon, however, are not well adapted to discover the part played by progressive understanding of the sense of a meaningful passage. In the first place, their tests were made only at the end of the learning period. We have already called attention to the fact that adequate analysis of the effect of distribution of time on intelligent and rote learning requires trial by trial comparison. But in experiments on memorizing, measurement of efficiency amounts in effect to additional practice, and other groups of subjects or sections of equivalent material must be used in each successive trial. The work required by either of the last two procedures has caused all experimenters to date to make measurements only at the end of the total period of practice. A second consideration also enters. Fox [17] has shown that verbatim reproduction is not well adapted to determine whether a subject has or has not understood a given passage. Lyon and Gordon required such word for word recall. Austin alone examined for 'ideas' remembered.

[17] Charles Fox, Educational psychology, London, 1925.

4. Massing should be more economical in earlier stages and distribution in later stages of learning. This follows from our postulate that achievement of insight is favored by immediate repetition, and the further deduction that a 'new' insight is more likely to occur in earlier rather than later phases of the learning process. A novel slant on a problem can indeed arise after a large number of trials, but would (by definition) necessarily be absent from the final stage of learning, for the reason that when a new understanding of the problem occurs, *it is not the same problem, and mechanization must begin over again.* Conversely, experiments with nonsense material, since they admit little or no progress in comprehension, begin at a 'late' stage of learning.

Obviously only experiments, in which trial by trial measurements of the economy of massed versus distributed procedure are made, can settle the question at issue here. Such comparisons have been made in mirror-tracing by Snoddy[18] and Lorge,[19] and in maze-learning by Carr[20] and Hardy.[21] Results for mirror-tracing show some form of distribution superior to massing in all stages of learning. However, since both Snoddy and Lorge found that, while 1 minute rest period gave much better results than no interval between trials, a 24 hour interval was only slightly more economical than 1 minute, therefore it would not be difficult to make conjectures about the relation between insight and fixing in mirror-tracing that would bring their data in line with our hypothesis. But such a course must savor of rationalization, since experiments in unambiguous types of learning can easily be carried out.

Hardy's research in maze-learning need not detain us, as his first 3 trials were used for equating the groups, and his shortest interval between trials was 12 hours. The investigation by Carr is more relevant. The latter had one group of 10 subjects trace a pencil maze 10 trials in immediate succes-

[18] George S. Snoddy, Learning and stability, *J. Appl. Psychol.*, 1926, 10, 1–36.

[19] I. Lorge, *op. cit.*, 12–18.

[20] H. Carr, Distribution of effort, *Psychol. Bull.*, 1919, 16, 26–28.

[21] M. S. Hardy, The effect of distribution of practice in learning a stylus maze, *J. Comp. Psychol.*, 1930, 10, 85–96.

sion followed by 1 trial a day for 10 days, and a second group (also of 10 subjects) trace the same maze 1 trial a day for 10 days succeeded by 10 massed trials. To the writer it seems that Carr obscured rather than clarified a comparison between the performance of his groups by this reversal of massed and distributed procedure in earlier and later trials with each group. If his groups were equal the shift was unnecessary, and if they were unequal the experiment was useless in any case. His results show no difference between massing and distribution by the time criterion, but a slight advantage for distribution in the early trials, when errors are considered. For the present we will only note the lack of agreement between Carr's data and our assumptions, and leave the possibility of a reconciliation to later discussion and experimentation.

A corollary to the postulation of greater effectiveness of distribution in earlier and massing in later trials is the deduction that massing should be more economical for immediate recall, and distribution for delayed recall. In fact, if our theoretical analysis is sound, the 'fixing' process has to do with retention rather than learning as such. Whatever the explanation, the data show that when recall is tested after a delay of several weeks distribution always has an advantage over massing, even with meaningful learning. In addition to Austin's results for recall after 1 day (reported above), which indicate equal efficiency for 5 readings at 1 sitting and for 1 reading a day for 5 days, that investigator found that when recall was delayed for 2 weeks or 4 weeks distributed procedure was three times as economical as massed procedure. Gordon's results were similar. For logical material with immediate recall massed procedure was uniformly superior to distributed, but for recall after 1 month the reverse was true. The relative economy of distributed practice in delayed recall is more marked when the material is meaningless. Robinson's comparison [22] of 5 minutes, 20 minutes, and 24 hours delay of recall for lists of three-place numbers shows this

[22] E. S. Robinson, The relative efficiencies of distributed and concentrated practice, *J. Exper. Psychol.*, 1921, 4, 327–343.

fact to some extent, but it is most clearly demonstrated by an
experiment of Perkins,[23] who tested recall 2 weeks after the
original learning.[24]   Sixteen repetitions of 7 pairs of nonsense
syllables were combined in 16 different ways: 1, 2, 4, or 8
readings of a list in a sitting, with 1, 2, 3, or 4 days interval for
each variation of the number of repetitions per sitting.   Since
the data indicate practical indentity for the 1, 2, 3, and 4
day intervals, we need only consider the distribution of read-
ings in different sittings (which is, of course, the reciprocal of
the number of readings a sitting).   The greatest degree of
massing, 8 readings in the first period followed by 8 readings
1, 2, 3, or 4 days later, gave an average of 13 percent correctly
recalled.   Four repetitions a day on 4 days, 2 repetitions a
a day on 8 days, and 1 repetition a day on 16 days, resulted
in an average (for the 1, 2, 3, and 4 day intervals) percent
correctly recalled of 32, 58, and 75 percent respectively.   One
repetition a day is thus six times as economical as 8 repetitions
a day.[25]

5. The complexity of the problem conditions the relative
efficiency of massed and distributed practice.   In default
of adequate experiment, only a very general statement can
be made at present.   The decisive factors governing the rela-
tion of complexity to time interval will be the ease or difficulty
of gaining an insight as to the problem, and, of course, the
stability of the configuration once attained.   All situations
so foreign to the individual's experience that comprehension
of the total pattern is impossible (within the time limits of the
experiment) are perforce excluded.   For all other problems
there should be a degree of complexity at which massing is
most efficient, and above (and below?) which its relative econ-
omy would decline until it is exceeded by some form of distri-
bution of practice.   This principle affords a possible basis of

[23] N. L. Perkins, The value of distributed repetitions in rote learning, *Brit. J.
Psychol.*, 1914, 7, 253–261.

[24] McGeoch (Robinson and Robinson, Readings in general psychology, Chicago,
1929, p. 390) in a momentary lapse from his usual accuracy, cites Perkins' experiment
as an illustration of the effects of massed and distributed practice on immediate recall.

[25] Perkins' procedure did not include 16 repetitions in a single sitting, but the
general trend of the data leave little doubt that this extreme degree of massing would
have been less efficient than 8 repetitions a day on 2 days.

agreement between our hypothesis and the data of Carr, Snoddy, and Lorge (previously discussed). Their problems may have been too complex to admit a grasp of the total pattern on a single trial sufficient to give any substantial aid in succeeding trials. The question is closely related to the whole-part problem, but so far the only comprehensive investigation of the interrelationships between whole-part learning and spaced-unspaced practice is that of Pechstein,[26] whose results are fully in accord with our assumptions. Pechstein discovered that with a simple maze part learning was most efficient by massed procedure, and whole learning by distributed procedure. The data by Hanawalt,[27] who found that with white rats whole learning was about 25 percent more economical than part learning, are only superficially contradictory to Pechstein's results, since the 'parts' of the patterns used in Hanawalt's investigation were more complex than the whole of Pechstein's maze. Hanawalt also used a combination of massed and distributed procedure that makes the conditions of her experiment difficult to analyze.

6. The intelligence and past experience of the subjects are additional factors.

In view of the wide-spread conviction voiced by McGeoch [28] that "some form of distribution is always superior to massed practice," the initial step in testing our theory is an experiment in which, so far as possible, all conditions will favor massing. If our assumptions are correct, by selecting a problem of a suitable degree of complexity and novelty and yet having a solution sufficiently integrated with systems of relationships already mastered by the subject to permit some comprehension of the pattern on a single trial, and with enough instability or lack of clarity in such comprehension to make it probable that the solution will not carry over the interval chosen for distributed procedure, it should be possible

[26] L. A. Pechstein, Whole versus part methods in motor learning, *Psychol. Monog.* 1917, 23, no. 99.

[27] E. Hanawalt, Whole and part methods in trial and error learning, *Comp. Psychol. Monog.*, 1931, 7, no. 5.

[28] J. McGeoch in Robinson and Robinson, Readings in general psychology, Chicago, 1929, p. 390.

to show a relative superiority for massed practice, at least in the first few trials. Measurements should be made after each trial, and retention tests taken some time after the initial learning in order to compare the effects of massed and distributed practice on immediate memory and retention.

Köhler's experiments with apes suggest that these conditions might be fulfilled for human beings by certain kinds of puzzles.

EXPERIMENT I

The puzzle shown at (A) in Fig. 1 is probably familiar to many readers. Since its name, if it has one, is unknown to the writer, it will be referred to throughout this discussion as the O-P puzzle. The adjustable parts of our model are blocks of three-ply wood one-quarter inch thick. The dimensions of the upper surface of all pieces is 1 × 2 in. with the exception of the large piece at the upper left and the two
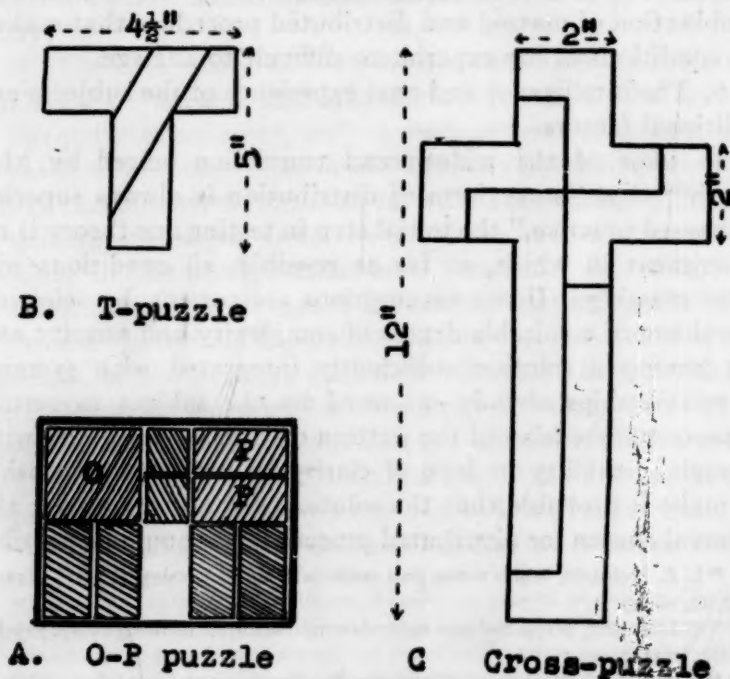
FIG. 1. Top view of puzzles.

small central pieces, which are 2 × 2 in. and 1 × 1 in. respectively. The blocks are contained in a shallow box 5⅛ × 6¼ in. inside measurement, as indicated by the heavy black line in Fig. 1, A. The problem is to interchange block 'O' for the two blocks 'P' by moving the 9 pieces into various combinations without lifting any from the floor of the box. To minimize friction the corners of the blocks were rounded slightly, and their lower surface and the inside of the box coated with floor wax.

Before the subject attempted a solution, the nature of the task was explained to him. A diagram of the puzzle with the pieces in final position was also placed before him and he was encouraged to check his orientation toward the goal by occasional reference to it. Nothing was said about speed, but the experimenter's stop watch doubtless suggested that time was an important factor. The experimenter also counted the moves. The average time and average number of moves on each trial, however, are so nearly parallel that only time records are given in the present paper. On the basis of performance in the first trial the subjects were divided into two groups. Group I solved the puzzle 20 times in a single sitting, with only sufficient time (about 10 seconds) between trials for the experimenter to record the time and number of moves and replace the pieces in the starting position (massed procedure). Group II had one trial a day for 20 days (distributed procedure). Subjects were asked not to think about the puzzle between trials. It was originally intended to secure another puzzle to be solved by Group I with distributed and Group II with massed practice. But the O-P puzzle proved surprisingly difficult. Some 35 subjects attempted a solution, and of these 21 gave up after working from one to two hours without success. Fourteen reached the goal (1 trial) after periods ranging from 22 min. to 109 min.

The average time in seconds for the 7 subjects (1 woman, 6 men) using massed procedure and the 7 subjects (3 women, 4 men) using distributed procedure appear at the left in Table I, and show decided superiority for massed over distributed practice for all twenty trials. The individual vari-

ability, however, is so great that no final conclusions can be
drawn from such a small number of cases.   Indeed, if medians
instead of means are taken, the advantage of massed over
distributed procedure is doubtful until the fifth trial.   Trials
5 to 13 show massing more economical, and after trial 14
there is no significant difference between the two methods.
The gross discrepancy between means and medians in the last
5 trials arises from the fact that one subject who followed
distributed procedure failed to reach a genuine insight into
the problem even after 20 trials.   His time records for the
last 4 trials were 825, 656, 444, and 519 sec.

Besides the difficulty of getting enough subjects to secure
statistically reliable results on a puzzle showing such wide
individual variability, two additional factors contributed to
the decision to suspend the investigation at this point.   In the
first place, there are indications that the O-P puzzle is too
complex to lend itself readily to analysis of the rôles of insight
and rote learning.   In the second place, any problem in which
'returns' are allowed permits a variable amount of massed
practice on certain parts of the problem.   If the solution can
be grasped only 'as a whole' such massing on parts has little
or no significance in the present connection.   The O-P
puzzle, however, is so constructed that partial insights can
and did occur before the final solution was obtained.   Obser-
vation of 35 subjects brought out distinctly the three-fold
character of the pattern: (1) Getting piece O to the bottom,
(2) Intermediate stage of manouvering the pieces into posi-
tion for: (3) Getting piece O to the top.   Each stage consisted
of a group of moves within which a subject might wander for
an indefinite period, but with only one set of key moves lead-
ing to each of the other stages.   And, once in a given stage, it
was just as difficult to get back to the preceding stage as to
reach the succeeding.   Subjects spent an average of about 10
min. in Stage I, and (in almost all cases) only a few sec. in
Stage III.   The great bulk of the working time and a vast
majority of the moves were thus made in Stage II.   Since the
goal could be reached in 60 moves and there are only a few
distinct patterns of right or wrong moves in Stage II, the

average of 4092 moves permitted clearing up and stabilization of the pattern of that stage in one 'trial.'

But massing practice in Stage I could also facilitate a grasp of Stage III, and careful study of the transition from Stage I to Stage II might aid one in understanding the transition from Stage II to Stage III.   In fact, as one subject demonstrated, a highly stable configuration of the total solution could be attained by this method in a single trial.   This possibility arose from the fact that Stage I is the reverse of Stage II.   The processes of getting Piece O to the bottom on one side of the board and to the top on the other side of the board bear a symmetrical relation to each other—they are idential as one's hands are identical.   Of course the transitions from Stage I to Stage II and from Stage II to Stage III are also symmetrical.   Subject S discovered this relationship and put it to use in a highly intelligent fashion.   He showed from the first a much better grasp of the pattern than any other subject,[29] and made no errors in Stage I.   Twenty-five minutes were spent in Stage II, at the end of which time he accidently hit on the reverse key moves between Stage II and Stage I, and returned to the starting position.   At this point something seemed to occur to him and *he went slowly and with careful attention over the moves of Stage I and the moves between Stage I and Stage II several times.   He then brought the pieces directly (through the already familiar Stage II) to the reverse position on the other side of the board and reached the solution without further errors or hesitation.*   I said nothing, but he volunteered the information as he rose from the table: "I think I could do it right away again.   I had an insight." (He had been reading Köhler's 'Mentality of Apes.')   His impression was correct.   The trial next day took only 60 sec., and he never afterwards lost his mastery of the problem.

The results of this subject, although he made the most rapid improvement of the 14 reagents, are not in any way contradictory to our hypothesis.   He achieved the solution of Stage III while working at Stage I, and felt the need of immediate repetition.   He therefore massed his practice of the solution before applying it in reaching the goal.

[29] He stated that he had no previous experience with the O–P or any similar puzzle.

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EXPERIMENT II

T-puzzle and Cross-puzzle

Experiment I indicated that analysis of the relation of the formation and fixing of a configuration to massed and distributed practice could be made best with a task (*a*) less complex than the O-P puzzle, and (*b*) intelligible only as a whole. For a second experiment, therefore, the T-puzzle and Cross-puzzle (Fig. 1, *B* and *C*) were selected. These seemed likely to be sufficiently simple and sufficiently unambiguous for the purpose in hand, since the structure of each puzzle can only be grasped as a whole. Preliminary tests, however, showed that memory of the first few solutions was transient.

The T-puzzle consisted of four pieces of heavy tin with a copper rivet soldered to the upper surface of each piece for convenience in handling. The five pieces of the Cross-puzzle were made of three-ply wood one-quarter inch thick. The shape and surface dimensions of the pieces are shown in Fig. 1, B and C. A shallow box held the T-puzzle, a rotating platform the Cross-puzzle. Both containers had hinged covers, and all parts were in duplicate so that in massed practice one model could be prepared while a subject was assembling the other. In each part of the experiment two students from the author's class in Psychology of Learning acted as observers: Miss Ethel Black and Miss Edna Sawler with the T-puzzle and Miss Clara Cook and Miss Rayworth Field with the Cross-puzzle. Detailed instructions and preliminary practice were given each pair of observers, and I have no reason to feel anything but confidence in their work. Twenty women students of Acadia University solved the T-puzzle, 10 subjects (Group III) had 20 trials in immediate succession (not more than 5 sec. elapsed between trials), and 10 subjects (Group IV) one trial a day for 20 days. With the Cross-puzzle Group IV had the massed and Group III the distributed practice,[30] but only 15 instead of 20 trials were given.

[30] One subject in each of the original groups working with the T-puzzle left college at mid-term, making it necessary to substitute two others in the trials with the Cross-puzzle. The inclusion or exclusion of the data from the unpaired subjects makes no difference in the trend of the results.

Retention tests of 5 massed trials were taken with the T-puzzle 4 weeks, and with the Cross-puzzle 2 weeks after the last trial of the practice series. For the T-puzzle, however, only 8 subjects took the retention tests and of these 3 subjects had only one trial.

Before the first trial each subject was seated in front of the apparatus, which rested on a table at a convenient height, and was shown the box or platform with the cover raised and the pieces in starting position. In order to favor mechanization in the final trials the pieces were placed in the same position before each trial. The subject was now given a card bearing the following typewritten instructions: "You are to put four pieces together to form a T" (or, 'five pieces together to form a cross'). "Go as fast as you can." The structure of the apparatus and the conditions of massed and spaced practice were then explained and the subject told that a trial began as soon as the cover was lifted from the puzzle. Timing was done by one experimenter with a stop-watch, while the other experimenter managed the apparatus, raising the cover at a signal to begin and closing the cover as soon as a solution was reached, and, in massed procedure, preparing one duplicate puzzle while the subject was working on the other. The experimenter's movements while arranging this duplicate were hidden from the subject by a screen. Subjects were requested to avoid all thought of the puzzles between trials.

The fact that Group III had massed and Group IV distributed practice with the T-puzzle, while Group IV had massed and Group III distributed practice with the Cross-puzzle, made it possible to nullify the influence of group differences in learning ability by combining the data from the two puzzles. However, as the two investigations gave like results, the measurements (time in sec.) for each puzzle are given separately. The means and medians appear in Table I and the means in Fig. 2. Since the trend of the results for trials 2 and 3 is the same as for the trials immediately following, the first three trials are omitted from the graph in order to permit plotting the data for the remaining trials on a larger scale than would otherwise be convenient.
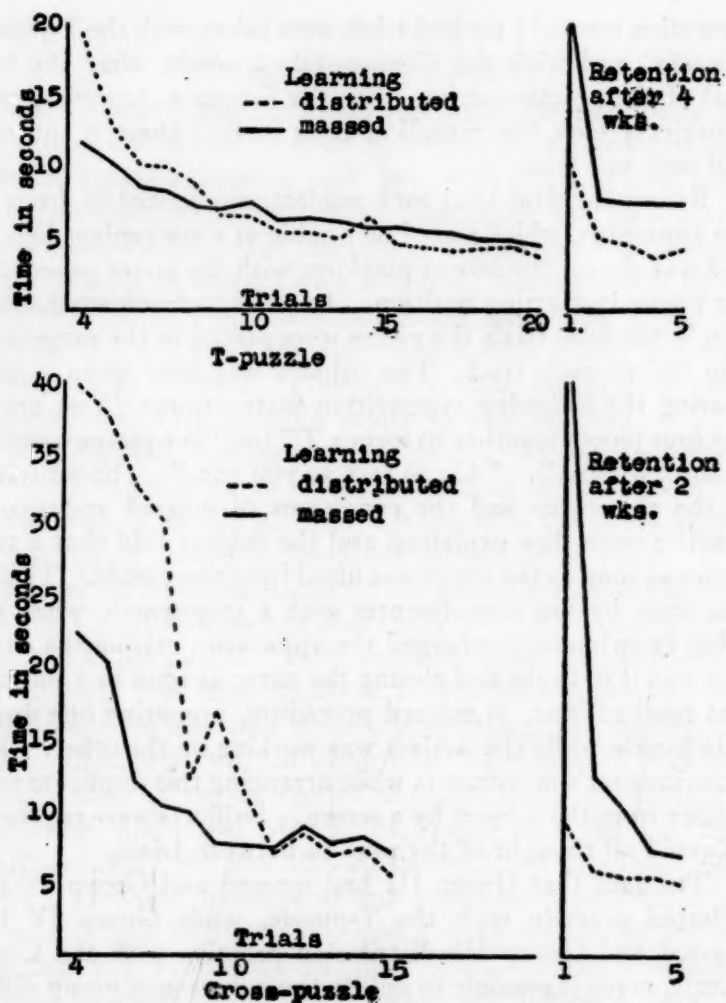
FIG. 2. The relative efficiency of massed and distributed practice on learning and retention in puzzle solving.

Table I shows that for these two puzzles massed practice is much more economical than distributed practice in the earlier trials. Because of great individual differences in the first few trials, it is necessary to take the combined records of T-puzzle and Cross-puzzle for trials 2 and 3 if averages are the measure, but the medians are unequivocal. The superiority

<div align="center">

TABLE I

AVERAGE TIME IN SECONDS REQUIRED TO SOLVE THE O–P PUZZLE, T-PUZZLE, AND
CROSS-PUZZLE

</div>

Trial	Practice											
	O–P puzzle				T-puzzle				Cross-puzzle			
	Massed		Distributed		Massed		Distributed		Massed		Distributed	
	av.	med.	av.	med.	av.	med.	av.	med.	av.	med.	av.	med.
1	4230	3655	4373	4530	535.7	341.0	239.0	248.5	638.9	624.1	612.1	478.5
2	1290	1290	2500	2450	71.2	37.6	246.6	49.7	211.3	35.7	233.0	95.2
3	656	307	731	250	77.7	19.0	41.4	35.4	24.3	14.0	75.1	42.7
4	193	202	302	98	11.8	9.6	19.1	13.1	22.5	12.6	39.8	28.8
5	142	106	289	160	10.3	9.0	12.9	11.7	20.6	10.7	37.8	10.1
6	69	69	264	105	8.7	7.0	10.2	10.1	12.3	11.4	32.8	18.5
7	72	49	263	102	8.2	6.9	10.0	9.5	10.6	9.0	30.3	13.0
8	55	42	221	77	7.3	6.3	9.0	7.6	10.0	8.3	12.5	9.2
9	56	47	279	52	7.0	6.1	6.8	6.9	7.9	7.2	17.0	9.2
10	51	41	327	48	7.5	6.0	6.7	6.4	7.9	7.5	11.3	9.0
11	52	35	174	75	6.2	5.7	5.6	5.5	7.3	6.5	7.1	7.2
12	47	35	279	44	6.4	6.4	5.7	5.6	9.0	6.8	8.7	7.2
13	42	37	171	56	6.2	5.8	5.0	5.0	7.9	6.7	7.0	6.0
14	115	47	138	43	5.8	5.6	6.5	5.1	8.2	5.6	7.6	6.2
15	45	35	140	34	5.9	5.1	4.8	4.6	7.2	7.2	5.7	5.4
16	40	36	74	35	5.4	5.1	4.5	4.5				
17	43	33	147	35	5.3	5.1	4.2	4.0				
18	69	33	125	30	4.9	4.9	4.4	4.7				
19	52	30	86	29	4.9	4.8	4.6	4.5				
20	54	36	100	28	4.6	4.2	3.9	4.0				
					Retention after 4 weeks				Retention after 2 weeks			
1					21.1	18.5	10.4	5.2	48.3	34.6	9.2	8.5
2					7.9	7.0	5.1	5.0	12.6	10.4	6.1	5.0
3					7.7	6.5	4.8	4.7	10.4	8.4	5.8	6.0
4					7.8	7.1	3.9	3.6	7.1	6.8	5.8	5.7
5					7.5	5.2	4.4	4.2	7.0	6.0	5.2	5.0

of massed practice gradually declines with each succeeding
trial until at trial 9 for the T-puzzle and trial 11 for the Cross-
puzzle distributed practice has the advantage, an advantage
which is not lost thereafter and is much more marked on
retention tests taken 2 weeks or 4 weeks later.

These results are wholly in accord with our provisional
hypothesis. Interpreted by its assumptions, the initial learn-
ing in solving the puzzles consisted in a discovery and grasp
of the relationships between parts of the pattern. Massed

practice favored the clarification and stabilizing of the novel configuration in the first trials. Distributed practice favored the fixing of the pattern as soon as it became sufficiently stable to be retained for 24 hours, and it favored to a much greater extent, the retention of the solution for longer periods.

Experiment II, though (I think) conclusive within its range, makes no pretentions to being the final word on the theoretical issue presented in this paper. An hypothesis of such far-reaching significance demands a series of experiments beyond the scope of the present paper, or even of one investigator. The brief outline of the interrelations of the dualistic theory with various factors usually assumed to be influential in learning, as well as the experimental data, are advanced merely as the starting point of a program of research (some of which is already under way), in the hope that other workers may find the theory of sufficient importance for independent verification or disproof.

General Discussion

In the interests of simplicity of exposition, I have so far discussed only those investigations bearing directly on our problem. However, a general trend that emerges in Ruch's analysis deserves some consideration. He states that: "Two studies using logical learning, one using rote, and two involving motor learning, indicate that distribution is most economical *in avoiding errors* rather than in making for rapid learning.[31] Two other experiments, one by Lashley [32] and one by McGinnis,[33] support this conclusion. Taken at their face value, these results seem incompatible with the part which we have assumed that insight plays in learning. Progress in intelligent comprehension of the total pattern of a situation should be at least as effective in avoiding mistakes as in improving speed. Closer examination of each of the four studies indicated by Ruch, however, somewhat resolves the conflict. Robinson's experiment with nonsense materials [34]

[31] T. C. Ruch, *op. cit.*, p. 37. Italics mine.

[32] K. S. Lashley, A causal factor in the relation of the distribution of practice to the rate of learning, *J. Anim. Behav.*, 1917, **7**, 139–142.

[33] E. McGinnis, The acquisition and interference of motor habits in young children, *Genet. Psychol. Monog.*, 1929, **6**, 203–211.

[34] E. S. Robinson, *op. cit.*

may be passed with brief mention, since the errors were not in the original performance, but in the recall. Whatever may be the mechanism of errors in recall of nonsense materials, it is probable that they are effected by factors extraneous to rather than inherent in the learning as such. Cumming's results [35] are not really contradictory to our theoretical position. His most economical procedure was a 'reducing' distribution, with massing in the initial stages followed by successively shorter periods and longer rest intervals. Under these conditions "a large number of determinations show massing in the initial stages to be somewhat advantageous if the criterion is amount, and much more so if the criterion is accuracy." [36] In Pyle's experiment [37] massed procedure comprised 10 half-hour periods of typewriting interspersed with half-hour rest periods to make a 10 hour day; distributed procedure consisted of 2 half-hour practice periods twice a day for 5 days. It is important to note that in neither procedure was there a consistent decrease in the number of errors. With distributed practice 313 errors were made in the first three and 337 errors in the last three practice periods. We may speculate concerning the causes of the lack of improvement in accuracy by either method,[38] but the important thing in the present connection is that we need advance no hypothesis to account for any supposedly greater effect of distributed over massed practice in deletion of errors, since there is no improvement in accuracy to account for. The decrease in accuracy in massed procedure may plausibly be attributed to fatigue, which might well be of importance in typewriting five hours out of ten. It has already been indicated, also, that such studies as Pyle's are only remotely related to our theory, since the so-called distributed procedure had one half-hour of work as its unit, and thus contained a great deal

[35] R. A. Cummings, Improvement and the distribution of practice, *Teachers College Columbia Univ. Contrib. Educ.*, 1919, no. 97.

[36] T. C. Ruch, *op. cit.*, p. 42.

[37] W. H. Pyle, Concentrated vs. distributed practice, *J. Educ. Psychol.*, 1915, 5, 247–258.

[38] Possibly connected with the fact that mistakes were not corrected. The data should have some significance for Dunlap's 'Beta' hypothesis.

of massing on individual 'associations.' A possible reconciliation of Carr's data and the view advocated in this paper has been already advanced on the basis of the relation of complexity of problem to the difficulty of getting a clear comprehension of its total structure. Without such comprehension massed practice loses any advantage it may possess.

The findings of Lashley and later confirmation by McGinnis may be explained in the same way. These investigators made a comparison of duplicate and novel errors in maze learning, and found a tendency for fewer duplicate errors to appear in trials separated by an interval. The key to an explanation is that immediate repetition favors the elimination of an error only if that error is perceived as such. A human adult will learn to take the correct path in a simple maze of the Y type after one exploration of the pattern, if a second trial is given immediately. If the second attempt is postponed for a week he may well have forgotten the correct turn. A comparison of novel and duplicate errors on simpler mazes would in all probability show results the reverse of Lashley's. Our argument thus involves two assumptions: (*a*) Errors may be avoided through insight if the relation of these errors to the total pattern is grasped and remembered on the next trial. This sort of elimination of errors is favored by massed practice; (*b*) errors may be deleted by other factors that are more effective in distributed practice. In mazes too complex to be grasped as a whole such factors would be preponderant and could give the results found by Lashley and McGinnis: more duplicate errors in immediately repeated trials.

It should also be remembered that maze-learning provides highly artificial conditions favorable to the dropping out of errors with a minimum of insight into the total structure of the situation. The number of errors is narrowly restricted and it is not only possible to reach the goal by mere blind trial and error, but conceivable that the error of entering each blind alley might be 'stamped out' by some sort of 'negative conditioning,' without regard to the total pattern. That the total pattern does to some extent control the learning process

even in mastering a maze has been of late sufficiently demon-
strated, but to see how narrow limitation of possible move-
ments and sharp check-up of each individual cul-de-sac
error aids piecemeal deletion of deviations from the correct
path, we need only refer to the negative results without such
help in Köhler's tests of box stacking in chimpanzees.   There,
in spite of persistent attempts at building over a period of
years, and success in the sense that the animals reached the
goal, there was no improvement in placing the boxes in a
stable position.   In this particular experiment the apes could
obtain the food without a clear comprehension of the total
structure of the situation, but, as Köhler has demonstrated,
it is not difficult to arrange situations in which such insight is
decisive.   That the rapid disappearance of 'random' move-
ments in early trials with the Cross-puzzle and T-puzzle is due
to the clarification of the pattern and thus to the domination
of the correct actions rather than stamping out of the wrong
movements will, I think, be sufficiently evident to anyone
trying the experiment on himself or another as subject.

If other factors than insight can effect the elimination of
errors, does not this conflict with our assumption of only two
kinds of learning: acquiring a novel configuration and fixing
that configuration?   The obvious answer to this plausible
objection is the assumption of an 'unfixing' process, that is,
the hypothesis that certain external factors favor the setting
of a configuration and others its disappearance.   This assumes
reversibility of the process making for retentivity, but no
difference in kind between 'fixing' and 'unfixing.'

It is highly probable, however, that our 'dualistic' theory
over-simplifies the learning process.   In any attempt to
distinguish types of learning it is necessary to choose a view-
point from which to derive a classification of the phenomena.
We have reversed the Behavioristic tradition, which starts
from overt behavior, and followed McDougall and the Gestalt
school in taking our principles from introspection.   The dis-
tinction between insight and fixing primarily concerns inner
changes, and, on the subjective plane, corresponds to Dunlap's
separation of 'responses' that change from trial to trial (the

change in a favorable direction being the learning), and 'responses' that remain approximately the same, and for which learning consists in increased probability of their recurrence. The growth of insight, or progress in comprehension, is a process of continuous change; while the process of fixing, considered subjectively, concerns the preservation of the new configuration, and is evidenced by the increased probability of the reappearance of that configuration as a conscious process.

Although both insight and fixing are effective in learning all overt movements, we do not need refined laboratory technique to show that the conditions governing conscious recall and recognition and the factors that determine the recurrence or change in the pattern of a movement are not always the same. The most striking example of this is the contrast between the effect of desire and aversion, or, if you like, of pleasure and displeasure upon memory and action respectively. An action whose performance results in a strong aversion, dissatisfaction, or pain is not repeated. But is the memory less vivid on that account? The burnt child dreads the fire, but he does not forget it. The memory of one who avoids a blind alley may indeed be better than that of one who enters the alley. In fact, while learning a maze by the aid of insight, memory of the blind alleys is (in early stages) a distinct aid to successful performance.

It remains to be shown that puzzle-solving is chiefly the appearance and growth of insight, and that motor processes as such have little to do with its genesis. It should be clear that such is the case with our puzzles. All movements involved are well-known and at the immediate command of the subject. Once he 'knows how' the movements follow from the structure of the configuration. For additional illustration an experiment of Lashley's [39] is pertinent. By an operation he paralyzed a monkey's arm and had the animal solve a puzzle with the other. Yet when the injured arm healed the monkey used it in doing the puzzle with little hesitation or

[39] Summarized by R. H. Wheeler, and F. T. Perkins, Principles of mental development, New York, 1932, pp. 65-66.

awkwardness. Could one doubt that a like experiment on human subjects with the O-P., T-., and Cross-puzzle would yield identical results? And this without prejudice to the probability that later stages in the mastery of such problems involve factors specific to the muscles concerned.

[MS. received February 3, 1934]

ORGANIC PSYCHOLOGY II: THE PSYCHOLOGICAL ORGANISM[1]

BY HULSEY CASON

University of Wisconsin

THE ORGANISM AS A WHOLE

The most familiar suggestion about the organism in psychology is that psychology studies the organism as a whole and that physiology studies only partial functions, but physiology frequently takes account of complicated pattern activities and such psychological processes as sensations and reflexes involve only limited portions of the body. It is impossible to study the whole organism at once in either psychology or physiology, and all scientific investigations in both fields are concerned with partial functions. The organism as a whole is an ideal which is never attained, but it is a useful concept and one which is involved in almost all of our theoretical discussions.

Every animal has a certain amount of unity because wherever he goes he carries all of the structures of his body with him, and many of the physiological and psychological functions of an individual's body are actually or potentially present all of the time. The always-present anatomy is a constant influence on the physiological and psychological functions, the physiological processes are influencing the psychological processes to some extent all of the time, and various psychological activities have a mutual influence on each other.

The description of animal bodies from the biological point of view disregards a number of characteristics which are included in the concept of the psychological organism. People may be similar to each other in their visible gross anatomy and known physiology but still differ radically in their psy-

[1] Part I is being published in the *Brit. J. Psychol.* (*Gen. Sect.*) and Part III in the *J. Gen. Psychol.*

chological organizations. Animals with similar sense organs perceive differently, and people with similar muscles do different things. The gross structure and as far as it is known the minute structure of the nervous systems of different people may be quite similar, but these people may still think, feel, and act differently. There are some anatomical and physiological differences between Martin Luther, Abraham Lincoln, Charles Darwin, Woodrow Wilson, Nikolai Lenin, Warren G. Harding, Benito Mussolini, and Herbert Hoover, but these anatomical and physiological differences are not important. The most interesting and significant differences between these men are differences in thinking, feeling, and acting. They are differences in the psychological organism.

THE ORIGIN OF THE PSYCHOLOGICAL ORGANISM

At the time of conception the structure and functions of the organism have already been influenced by a number of physical and biological factors. The organism such as it is at conception does not contain all of the causes of its own later development because the growth of the organism between conception and birth is influenced by the interaction between the organism and its environment. This interaction is well described by Professor C. M. Child. "The organism is not at any stage a closed system, but is functioning and behaving at all times as long as it is alive. Reaction to environment is occurring at all stages of development, though of course the kinds and complexity of reaction differ at different stages according to the mechanisms present. Moreover, such behavior or reaction is itself a factor in development and therefore in the construction of the behavior mechanisms of later stages. The behavior of the various developmental stages as well as the specific hereditary constitution of the protoplasm is a factor in determining the behavior of the fully developed organism" (4, p. 2).

A consideration of the origin of the organism is one of the safest guarantees that the extent of the influence of some particular structure or function will not be overestimated. There is a special danger that the influence of hereditary fac-

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tors will be misunderstood because some sociologists and technical geneticists have held such contradictory views in recent years. Some geneticists still claim that all of the significant characteristics of the individual are potentially present in the germ plasm when the individual begins his life, and one is almost asked by some of the environmentalists to believe that a person is like a lump of putty which is shaped and moulded in a passive way by social forces. Many geneticists and sociologists completely disregard factors which are stressed by their opponents and several of them even disregard essential features of the psychological organism.

The organism is influenced by factors which are hereditary, by organic factors within the organism, by environmental factors, and at a later age by social factors. All of these factors have some part in producing the organism, and none of them can be disregarded. Practically all of the energy of all kinds which comes to exist in the organism is in the last analysis obtained from the environment. The organism receives energy from the environment and gives it back in a different form, and the organism becomes at successive life stages an increasingly significant system of energy exchange. The germ plasm is not the sole cause of the organism because there is no time when the organism can live without certain environmental influences. One cannot regard the social environment as the primary cause of the organism because before the individual is born there is no social environment. A biological organism must exist before a psychological organism is possible, but a psychological organism can live in almost any kind of cultural environment.

In addition to the interaction between the organism and its environment both before and after birth, there is a mutual influence of many bodily structures and functions on each other. There are many interactions between pattern activities, but there is no single feature which can be regarded as the sole cause of all of the other structures and functions of the organism. Sensory, nervous, muscular, conscious, and unconscious functions cannot be regarded as the sole or primary causal agencies because there was a time when none of

them existed and all of them are the results of previous conditions. One cannot consider the structural features of the organism as the sole cause of the functional because it has been shown that physiological and psychological processes can produce changes in gross anatomy and in the chemical composition of the cells.

THE ORGANIC FRAMEWORK OF PSYCHOLOGICAL ACTIVITIES

There has been some preoccupation in psychology with attempting to relate all kinds of activities to a theoretical framework of sensory, nervous, and muscular functions, and this preoccupation has caused some neglect of other functions. The physiological functions of excitability, conductivity, and motility are a part of an organic system of physical and chemical integrations and disintegrations, the extreme complexity of which has been suspected but not completely understood. The vital processes of nutrition, growth, regulation, and repair are closely related to these physical and chemical activities. Salts, sugars, and fats are passively transported in the blood stream in solution or in suspension and the bodily effect that is produced is in some way associated with the physico-chemical constitution of the substance transported. The hormones of the glands of internal secretion also produce changes in the body and these changes are related to the chemical constitution of the hormones.

In the organic framework which we have in mind, there may be a transfer of energy rather than a mass transport of material, and ions or electrons may be transported from one molecule or atom to another. There may also be a direct mechanical transmission of pressure and tension as well as a conduction and transmission of heat and electricity. Conduction of heat occurs in the regulation of bodily temperature, and some conduction of electricity is taking place all of the time.

The physical and chemical functions of the organism are of special interest in physiology, but they are also related to all kinds of psychological processes. They may not be involved in such psychological processes as fatigue and the affectivities

in the same way that they are involved in physiological processes, but they are still involved in the psychological processes. It would seem that the mere knowledge that the framework of all psychological activities is an organic framework would constitute a step toward an ultimate understanding of psychological activities and of the nature of psychological science in general.

STIMULUS AND RESPONSE

The concepts of the stimulus and the response were taken over in psychology from physiology, and whatever artificialities may have resulted from the transplantation to the new environment have been due to the special emphasis on sensory, nervous, and muscular functions and the failure to take account of certain features of the total organic framework which have been considered more carefully in physiology.

In sensory-neuro-motor processes what is needed is not so much a change to the conscious perceptual experiences of the Gestalt psychologists as a return to the old and conservative physiological principles of interpretation partly in terms of organic patterns. The concepts of stimulus and response are widely used in both physiology and psychology, and there should be no special qualitative difference in the way these concepts are understood in the two sciences.

A stimulus is commonly defined as a mechanical, thermal, chemical, or electrical energy change which has some influence on a sense organ, and since the sense organs are parts of the body, all stimuli must be within or at least in contact with the body. Most of the stimuli which have some influence on vital functions are located inside of the body and the large majority of these internal stimuli as well as the responses associated with them are unconscious in their action. The organism is stimulating itself in one way or another all of the time regardless of whether the individual is conscious or not. Stimuli acting within or on the surface of the body may sometimes be regarded as initiating agencies for the activities which follow, but they cannot be considered the sole causes of these activities because the organism itself is a complex system of chains and patterns of causal events.

When we say that the general framework of the organism influences the processes which occur in the organism we are not thinking about the biological organism alone. A person becomes a psychological organism at a relatively early age, and he is much more than an anatomical or physiological specimen long before maturity is reached. He acquires elaborate verbal, perceptual, motor, and affective habits; he has conscious processes which influence many different kinds of behavior; and he has other psychological traits and capacities which make him different from the lower animals. External stimuli alone do not determine how he thinks, feels, and acts. He actively seeks those forms of stimulation which please him, and avoids other forms of stimulation which he does not like. He is a psychological organism capable under certain conditions of exerting a powerful influence on the world. He selects many of his stimuli and also many of his responses. He does not merely react in a passive way to external stimuli, and experience is not just called out in him. As Max Eastman says, he seeks experience.

The psychological organism must be considered in all studies of the relation between stimuli and responses. A number of stimuli are always affecting the organism, many of these being internal stimuli, and a number of responses are always occurring in the organism. The internal patterns of stimuli are generally quite complicated, and every response occurs in an organic system of other responses. The concepts of a single stimulus and a single response may therefore be regarded as abstractions which are perhaps never realized in actual practise. The influence of a limited number of stimuli can only be studied when the effects of other stimuli are in some measure equalized or held constant.

Since more than one stimulus is always affecting the organism and since more than one response is always occurring in the organism, it may be desirable to use the word 'situation' for the whole pattern of internal and external stimuli. If the total situation is the same on two occasions, it is justifiable to assume that the total pattern of responses will also be the same; or, more briefly stated, the same situation will always call out the same response.

One of the principal aims of psychology is to determine the antecedent situations or causes and the consequent responses or results in all kinds of psychological activities. If the necessary data on the situation-response connections are available, the behavior of an individual can be predicted from a knowledge of the situation in which he is placed, and a desired form of behavior may be produced by arranging the situation in the appropriate way. It is of some value to determine the relations between the external stimuli and the externally observable modes of behavior, but it is also desirable and in many cases necessary to consider what is taking place within the organism itself. One of the most important of these implicit activities is inner speech.

The Reflex Arc

The analytical study of behavior generally leads to a consideration of the reflex arc even though it is admitted that the simple reflex is an abstraction. A reflex is one of the less complicated modes of behavior involving a somewhat limited portion of the body, and in contrast with other functions of the body it is relatively automatic and permanent. The pupillary, eyelid, and knee-jerk reactions which are frequently given as examples of reflexes are much less complicated than the majority of reflexes. In reflexes the stimulus has a trigger action and releases energy that is stored in the body, and the total amount of energy expended in the response is generally much greater than that which is present in the stimulus.

The six principal structures or processes which are involved in the ordinary reflex arc are, in the order of their occurrence, as follows: (1) the stimulus, (2) the sense organ, (3) sensory or afferent nerves, (4) the central nervous system (spinal cord and brain), (5) motor or efferent nerves, (6) a muscle group or gland. When the sense organ is stimulated, nervous impulses pass from it over the sensory nerves to the central nervous system, and the reverberation that is produced in the central nervous system does not ordinarily die down without some further changes in the body. Several different structures are successively affected, and there are

also several changes in the form of the energy which are related to the structures and functions involved. Photic energy, for example, is entirely a matter of physics at first, and the refraction of light in the eye is both physical and physiological. The stimulation of the retina is both chemical and physiological. The structure and functions of the rods and cones of the retina are matters of cytology and histology. The nervous impulses from the retina are in the fields of nerve physiology and neurology, and the functions of certain muscle groups involved in the pattern reaction are principally studied in physiology. There is no single science which studies all of the organic processes involved in the simplest reflex arc.

Any consideration of a reflex should include detailed information in regard to the nature, size, shape, location, and functions of all of the structures that are involved. Reflexes depend upon the structure and location of the sense organs, upon the nature and arrangement of the nervous tissue, and upon the structure and location of the muscles or glands. The first consideration in the study of a reflex is the structure and functions of the sense organs. Different responses can be produced by stimulating the same sense organ in different ways, and differences in the quality, intensity, and duration of conscious activities may also be produced by different kinds of stimulation applied to the same sense organ.

Reflexes are influenced by the structure and functions of the nervous system, but the connection between the sense organs and the muscles or glands is always an indirect one through the afferent, central, and efferent nervous systems. There is considerable information about the structure of the peripheral and central nervous systems, and there is some information about the functions of the peripheral system. The central and peripheral nervous systems differ radically from each other in the number of nerve fibers and in the distribution of these fibers, and in spite of the claims of some nerve physiologists there are gross differences in function which are correlated with these gross differences in structure.

The responses of muscles and glands depend to some extent upon the condition of the muscles and glands themselves.

A muscle group may be in good or poor condition, and the chemical and other conditions of glands vary greatly at different times. Muscular activity is strongly influenced by afferent, central, and efferent nervous impulses, but the state of a muscle is a factor in its own behavior. The contraction of a muscle depends upon its nature, size, shape, and location; upon the nervous impulses which it sends to and receives from the central nervous system; and upon the force which the muscle has to pull against.

The functioning of a reflex depends upon all six of the factors which we have referred to above, from the stimulus to the motor or glandular response inclusive, and the response will fail if there is a failure in any one of these six factors. Psychological processes do not originate in the central nervous system, and no activity of any kind ordinarily occurs if there are no stimuli. With continued stimulation the efficiency of many sense organs decreases rapidly by the process of sensory adaptation, and nervous tissue especially in the central nervous system is also subject to fatigue. In addition to conditions which are the direct and immediate result of nerve action, the central nervous system is always being influenced by a variety of subtle and none too well understood, but still important, processes of a chemical nature.

Reflex action cannot be explained by or reduced to any one of the six factors which we have mentioned. Reflexes involve the stimulation of sense organs, but sensory processes are only a part of the total picture. Reflexes involve the activity of the central nervous system, but activity in the central nervous system is the result of stimulation by sensory nerves. When the sense organs or sensory nerves are injured, and especially when the individual is blind and deaf from birth, the structure and functions of the central nervous system are seriously impaired. Reflexes also involve the activity of muscles and glands, but responses do not ordinarily occur in muscles except when they are stimulated by nervous impulses.

We have described the general features of reflexes because they are involved in many psychological activities, but reflexes differ greatly among themselves, and several of the

simplest nervous reactions of the body, such as some of those involved in digestion, do not have the general characteristics of ordinary reflexes. In the early stages of development, the externally observable responses are somewhat diffuse and general in character; and in fetuses, the younger the fetus the more diffuse the reactions tend to be. Motor and sensory activity may be present before the nervous connections are established. The reflex connections of the spinal cord are strongly influenced by heredity, and these connections tend to be similar in different individuals of the same species; but the brain, which is relatively more important in higher animals and especially in man, is more variable and much more susceptible to environmental influences. The brain has different influences on the reflex behavior of different individuals. The formation of nervous connections by the process of learning is partly conditioned by the functions of the brain, and in many animals but especially in the case of man, reactions are commonly acquired which have all of the automatic and permanent characteristics of true reflexes.

The Psychological Organism and the Necessity of Scientific Analysis

The principal limitation of the reflex theory seems to be that under the ordinary conditions of life the stimulus is not the beginning and the response is not the end of the organic process. Organisms bring external stimuli to themselves and carry their sense organs within the range of external stimuli; but in contrast with the sense organs located on or near the surface of the body, many of the internal sense organs are in a relatively helpless condition as far as being stimulated is concerned. The kinesthetic sense organs in skeletal muscles are stimulated when these muscles contract, and they send impulses to the central nervous system which cause further responses in the nervous system, in other muscle fibers, and in the body in general. These responses generally change the later stimulating conditions of the organism and the later chains and patterns of organic responses. These complicated processes bear a closer resemblance to sensory-neuro-motor circuits than to simple reflex arcs.

Most physiological functions are more complicated than the simple reflex arc, and several functions are commonly involved in what may be considered for convenience a single bodily process. Respiration is influenced by chemical factors, digestion is partly a matter of what is in the stomach, heart action is always being influenced by a large number of factors, and even such responses as coughing and sneezing are not simple. The large majority of psychological activities are also much more complicated than the simple reflex arc. Walking involves the position of the body, the pressure on the ground, and the preceding sensory-neuro-motor activities. Listening, speaking, reading, and writing are elaborate functions, and such activities as native drives, emotions, interests, and habits are characterized by organic complexity. The nervous tissue involved in reflexes is not arranged in a neat and regular fashion, and peripheral nerves do not resemble the lines of a simple geometrical figure. The brain is an intricate mass of nerve fibers and supporting tissue which, even in the dead and inactive condition, defies adequate description. The best established fact about the functions of the brain is that they are complicated.

The organism is a useful frame of reference for all kinds of psychological activities, and it is frequently desirable to consider the relation of the particular activity being studied to the general characteristics of the individual. It is impossible to observe and experiment upon the whole organism at once, but the interrelations of different kinds of organic processes can be profitably kept in mind while an analytical study is being made of partial functions. The concept of the psychological organism is not opposed to the experimental method. It justifies this method and makes its use a necessity, because the essential feature of the experimental method is that observations are made on a limited number of factors while an attempt is made to equalize or control the influence of other factors.

REFERENCES

(The following references are suggested as being particularly important in connection with the present topic.)

1. BURNHAM, W. H., The significance of stimulation in the development of the nervous system, *Amer. J. Psychol.*, 1917, 28, 38-56.

2. CARR, H. A., Psychology, a study of mental activity, 1925, Chap. 4: Some principles of organic behavior.

3. CARR, HARVEY, Functionalism (Chap. 3 in Psychologies of 1930. Ed. by Carl Murchison, 1930).

4. CHILD, C. M., Physiological foundations of behavior, 1924, Chaps. 1–6 on organismic pattern and integration.

5. COGHILL, G. E., Anatomy and the problem of behavior, 1929.

6. DASHIELL, J. F., Fundamentals of objective psychology, 1928, Chap. 3: The analysis of behavior.

7. DEWEY, JOHN, The reflex arc concept in psychology, PSYCHOL. REV., 1896, **3**, 357–370.

8. EASTMAN, MAX, The will to live, *J. Phil., Psychol., & Sci. Methods*, 1917, **14**, 102–107.

9. HERRICK, C. J., Neurological foundations of animal behavior, 1924, Chap. 5: The vital energies.

10. IRWIN, O. C., The organismic hypothesis and differentiation of behavior: II. The reflex arc concept, PSYCHOL. REV., 1932, **39**, 189–202.

11. KANTOR, J. R., The psychology of reflex action, *Amer. J. Psychol.*, 1922, **33**, 19–42.

12. KANTOR, J. R., The nervous system, psychological fact or fiction, *J. Phil., Psychol., & Sci. Methods*, 1922, **19**, 38–49.

13. KANTOR, J. R., The organismic vs. the mentalistic attitude toward the nervous system, *Psychol. Bull.*, 1923, **20**, 684–692.

14. KANTOR, J. R., A survey of the science of psychology, 1933.

15. LADD, G. T., & WOODWORTH, R. S., Elements of physiological psychology, 2 ed., 1911, Chap. 7: Reflex functions of the nervous system.

16. McDOUGALL, WILLIAM, Outline of psychology, 1923, Chap. 2: The behavior of the lower animals.

17. MINKOWSKI, M., Über frühzeitige Bewegungen, Reflexe und muskeläre Reaktionen beim menschlichen Fötus und ihre Beziehungen zum fötalen Nerven- und Muskelsystem, *Schweiz. Med. Wochenschr.*, 1922, **3**, 721–724, 751–755.

18. PRATT, K. C., NELSON, A. K., & SUN, K. H., The behavior of the newborn infant, *Ohio State Univ. Stud. in Psychol.*, 1930, No. 10.

19. THURSTONE, L. L., The nature of intelligence, 1924.

20. WARREN, H. C., Psychology and the central nervous system, PSYCHOL. REV., 1921, **28**, 249–269.

21. WARREN, H. C., Neurology: mystical and magical, *Psychol. Bull.*, 1923, **20**, 438–443.

22. WEISS, A. P., Conscious behavior, *J. Phil., Psychol., & Sci. Methods*, 1918, **15**, 631–641.

23. WHEELER, R. H., The science of psychology, an introductory study, 1929, Chap. 17: The nervous system in its relation to behavior.

24. WHEELER, R. H., & PERKINS, F. T., Principles of mental development, a textbook in educational psychology, 1932, Chap. 2: The laws of human nature.

CAN AN ECLECTIC POSITION BE SOUND?

BY RICHARD WELLINGTON HUSBAND

University of Wisconsin

I. Problems

The fact that there are a number of schools of Psychology which differ from one another in their fundamental premises has recently excited a certain amount of comment. Several decades ago psychologists were not divided into camps in this way. The field then obtained most of its data through introspection, most of the laboratory situations being designed to provide a setting for this introspection. Around 1910 a new school, Behaviorism, arose and challenged many of the premises of the then existent psychology, which we now term Structuralism. In turn Gestalt finds flaws in both Behaviorism and Structuralism. In addition we find advocates of Psychoanalysis, several types of Purposivism, and a few less known schools.

Observations, experimental results, and above all the outcome of certain 'crucial' tests have been seized upon avidly as substantiating the premises of these various systems. Accordingly a large number of psychologists have taken positions, accepting in whole or in large measure the teachings of one school or another. One may deduce such acceptance from specific writings on theoretical topics, by interpretation or inference from their discussion of experimental results, or indirectly from personal conversation.

Lately, however, a number of writers have taken exception to this tendency to allegiance with one school to the exclusion of the facts and major theories of the others. Woodworth (7) advises us to be broad-minded and walk down the middle of the road, instead of narrowing our range of vision by keeping to one side. Stevenson Smith (5) professes to be highly annoyed when accused of being behavioristic, that is, of following one special system.

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At first hearing, arguments against adopting any single school sound very logical. Eclecticism has a profound sound and seems to give one a badge of broad-mindedness. One does not have to limit himself to any one school, but rather can select the best from each, and in this way form a mosaic which promises to be far wider in scope than is any single viewpoint.

However, the writer has serious doubts as to whether such a program can be carried out without having serious inconsistencies among one's fundamental assumptions. It is from this argument that I intend to develop my thesis. In our treatment we shall first state the arguments for Eclecticism, and then examine the nature and demands of systems and fundamental premises, to see if those arguments are tenable.

II. Arguments For Eclecticism

We quote five arguments on behalf of Eclecticism. These in turn seem to be grouped around two main lines. The first suggests that one select what is best from each school and incorporate those elements into his system. The second advises one to hold to a compromise or neutral view on disputed topics, rather than go to one extreme or another.

1. *Selection from Various Systems.*—To illustrate this standpoint we quote from Boring. Here Boring has laid his finger on one difficulty of Eclecticism: that of selecting the best. Who can say what is the best? The second paragraph, taken from Woodworth, shows the same view expressed somewhat more in detail.

Mere eclecticism has no single point of view. It is a 'choosing of the best,' and since there can be established no absolute 'good' with the schools in such sharp disagreement, the 'best' must remain individual and personal. . . . Even the eclectic must choose, and in this case he chooses what has proved its worth [Boring (1), pp. 115–116].

If the existentialist presents a good analysis of heat sensations, or of color experiences, we accept it with thanks. If the behaviorist shows by experiments on little children how conditioned fears may arise, we are free to use that finding in our own psychology. If the Gestalt psychologist should show us that all learning depended on

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some degree of insight, we should revise our conceptions of learning accordingly. If the purposivist convinces us that the individual is never passive when a stimulus reaches him, that is another important point to be dealt with. If the psychoanalyst opens our eyes to the importance of sex motivation, we thank him for that. Every genuine positive result is meat for the psychologist who is not prejudiced by loyalty to a particular brand of psychology [Woodworth (7), p. 214].

We shall attempt to show later that such a procedure will create inescapable disagreements when one tries to reduce further or to reconcile his fundamentals. It may be argued that our premises are too uncertain as yet to try to hold to a uniform system, but it is likewise dangerous to start something from which later retreat is inevitable.

2. *Compromise between Systems.*—Several writers have suggested that a compromise view on single topics be taken. We quote from Woodworth and Klein, respectively.

I can imagine, for instance, that goal-seeking, insight, and the conditioned reflex may all be found to be indispensable in the process of learning [Woodworth (7), p. 218].

This entire issue can be summed up adequately by taking note of a specific problem, upon which much light has been shed, such as that of space perception. It would be somewhat absurd to venture to reduce this problem to a single category of explanation. A complete account of the parts played by all the known factors operative here would require consideration of every one of the explanatory principles mentioned. To make the phenomena in question exclusively a configurational affair, or solely a matter of redintegration, or wholly a mere linkage of the retina to occipital tissue, or predominantly a matter of ideational associations would be a ridiculous performance. The result would be either an incomplete account of space perception or a very much distorted one. Some aspects of such perception are best accounted for in terms of one principle and other aspects require different principles. The task of the psychologist is to assemble the data and apply relevant principles to the appropriate aspects, recognizing that these principles are supplementary and not contradictory [Klein (3), p. 495].

But is it possible to combine the atomism of structuralism and behaviorism together with the relational and configurational aspects of Gestalt? As to this Klein says:

Judicious eclecticism in the choice and application of explanatory principles even if this means an unfinished science seems to us to be preferable to partisan attachment to a single principle in the interests of a finished system [(3), p. 496].

Expediency thus triumphs over fundamental agreement.

Brown (2), in a rebuttal to Klein, has attributed much of the dispute to the fact that psychology is a very young science. He instances a number of severe controversies which have been and still are raging in physics. *Certain assumptions demand certain systems of thought, and they must remain separate until some are proved incorrect or until still more fundamental premises are discovered which demonstrate a common basis to all.*

(3) *Single Systems Distort the Truth.*—In following any single system we do not get the whole truth, and often completeness is attained only by juggling and warping facts to fit in with preconceptions. The Gestalt school is an example of this. Perceptual analogies, usually visual, are used to demonstrate their principles in all fields of behavior. Freud's ardent search for sex conflicts is another instance. Hollingworth's redintegration hypothesis, although not intended to be a complete system, seems to be stretched far beyond normal elastic powers.

In reporting experimental results one must be very careful to avoid distortion and interpretations beyond the facts. Complete objectivity and conservatism are the very fundamentals of science, and if violated for systematic reasons the science itself vanishes.

Perhaps one of the most important reasons for differences in viewpoint is that different materials are studied by the various schools. This can be illustrated by an example away from theoretical psychology. Occasionally when students are taking courses simultaneously in biology and sociology, they are bothered by the fact that one department stresses heredity, while the other teaches that environment is all-powerful. The apparent contradiction is reconciled when one discovers that one field deals with physical structure, while the other is speaking of social relations. At times the

situation is even worse than this; the same set of facts is interpreted in two ways. Koffka claims that some of the curves by which Thorndike attempted to demonstrate stupidity in the animal actually show insight. Where it does not appear, he says it is not due to the animal's inability, but rather to the experimenter's setting up a situation which precludes the use of insight.

This last statement suggests a clue as to one important cause of differences between schools. By the experimental situations they set up, they may be producing results which are bound to fit in with and substantiate their premises. The more this type of research is conducted, the wider will be the gaps between viewpoints, since each theory receives more and more confirmation. The various schools are actually speaking different languages, and at times may fail to understand others who are expressing the same thought in different terms.

4. *Some Material Is Common to All Schools.*—Stevenson Smith [(5), pp. 461–462] says:

The experimental attack is not different in different schools· What the behaviorist (and others) has achieved is due to his being a good experimentalist and in spite of his philosophy. . . . Whatever results survive from present-day configurationism will have a physiological, not an occult, explanation. The structuralism of the future will be independent of any philosophy but the philosophy of science, and its data will be inspected only for their reliability.

The present writer would disagree to some extent with this prediction of Smith's. Experimental method in general lays down only rules on carefulness of attack and verification, not the materials, apparatus, or problems which should be treated. So we can and do have various schools all founded on reliably ascertained facts, but dealing with entirely different problems. We can not say one is correct and the other false, any more than we would criticize the vision of a friend who is looking out of a window from an entirely different angle than ourselves and consequently sees entirely different objects.

5. *Many Leaders in Psychology Belong to No Particular*

School.—Woodworth makes a very strong argument of this, claiming that only six of thirty-seven past presidents of the American Psychological Association (up to 1928) were members of any definite school of psychology. This rating would depend somewhat on the strictness of the criterion, as the reviewer made a list for himself and judged that at least sixteen had definite systematic inclinations.

Not every leading psychologist, however, has expressed systematic tendencies as such. In some cases outlooks may be inferred very definitely from his writings. Some men are purely experimentalists, have no systematic leanings, and apparently are not particularly concerned with them. Dodge and Miles are examples. Others set up experimental situations to substantiate their hypotheses—Tolman, Köhler, Watson. Still other bits of research which have been undertaken for the sake of fact alone may be pounced upon eagerly by others who see in them data which support their theories. Researches by Lashley, Pavlov, Carmichael, and Coghill have received tremendous publicity, not so much because the facts themselves have far-reaching interest, but because of the theoretical implications underlying their findings.

III. What Is a System?

What is a system? What does adopting one mean? How does the possession of a system affect a man's outlook? These questions must be answered before one can properly evaluate an eclectic position.

McGeoch [(4), p. 2] gives this definition: "By the term 'psychological system' is implied a coherent and inclusive, yet flexible, organization and interpretation of the facts and special theories of the subject." In more general terms this means that one's views on the various important theoretical topics must spring from the same fundamental assumptions, must agree with one another where they meet, must be mutually compatible, and must cover the whole field.

The search for fundamentals by which to evolve a consistent explanation of the problems and facts of the subject is not confined to psychology alone. Since the time of the an-

cient Greeks scientists have been searching for the ultimates of matter. The long road from Empedocles' earth, air, fire, and water through the atomic theory to that of electrons and protons has not been completed as yet. There is plenty of difficulty harmonizing Newton's rigid gravitational system, Einstein's relativity, and quantum mechanics.

Problems which are fundamental in Psychology are: Mechanism and Vitalism; Body and Mind; Epistemology; the nature of Consciousness; and the validity of Introspection. Theories underlying Instincts, Emotions, Learning, and Thinking are more particular, but important.

One's beliefs on these topics must influence his thinking in many ways, and so fundamental are most of them that the beliefs must agree with each other, in order for a person to be consistent.

Does one need to adopt a system? Does he have to be a Behaviorist, a Gestalter, or a believer in Psychoanalysis? The chief argument of this paper is that he must follow one of these systems, some other, or have a consistent one of his own, if his thinking is carried on deeply enough.

This is not to be interpreted as meaning that one is urged to choose a system and to force all of his thinking into its dictates. This is a serious mistake. It is far better for him to think out each problem in turn; but I believe that if he does so he will find his system forming itself. If his ultimates do not agree, there must be some faulty or incomplete thinking about one or another problem. He may be temporarily baffled and not be able as yet to solve all topics to his satisfaction. An example is mechanism; one may reject vitalism in learning and instinct, but be unable to give a complete explanation of will on mechanistic grounds. But this does not mean that he has to align himself with the Eclectics; rather he is temporarily baffled and prefers to reserve his decision until more information is at his disposal and his thinking is more advanced.

IV. Fundamentals of Present Leading Systems

To give a basis for our contention that one's thinking must be self-consistent, let us state briefly the fundamental points of several of the leading systems of the present.

1. *Behaviorism* has a fundamental premise of movement, or behavior, studied strictly objectively, and interpreted in terms of stimulus and response. In its extreme form it assumes mechanism and physical monism.

2. *Gestalt* psychology has a negative premise in its protest against the atomism of structuralism and behaviorism, and a positive premise in pointing out the relational and non-absolute aspects of behavior, particularly in the field of perception.

3. *Psychoanalysis* emphasizes the driving force of instincts, which keep the individual normal when properly integrated, but which cause abnormal behavior when repressed or improperly balanced. It is an incomplete system, leaving out intelligence and most phases of learning and memory; in fact almost all problems outside of personality and abnormal phenomena.

4. *Structuralism* studies behavior through an atomistic reduction of consciousness by introspection into sensations, images, and feelings. It is rooted in dualism, which gives a clue to interpretation of many of its doctrines.

V. CONSEQUENTS OF A FUNDAMENTAL ASSUMPTION

Let us illustrate by means of Behaviorism the far-reaching consequences of making certain fundamental assumptions.

This school attempts to be thoroughly mechanistic and objective. Immediately we can predict their stand on several important topics: soul, will, consciousness, and introspection. They have no use for the soul, as there is not the slightest shred of evidence to convince us that it exists at all beyond the negative argument that life has not as yet been explained or duplicated completely mechanistically. The will is discarded, as it is mysterious and not amenable to physiological explanation. Consciousness has been discarded, or ignored, for the same reason. Introspection is not considered a proper source of psychological information, as it is subjective, unverifiable, of unknown reliability, and deals with inconsequential processes. These omissions may leave serious gaps, but it is not our place to attempt to evaluate the arguments.

The problem of the mind comes in for extremely interesting discussion. The concept, it is said, is as gratuitous and vitalistic as are those mentioned in the preceding paragraph. The term mind is often employed as a sort of functional concept to indicate the workings of the structure, the brain, in particular the cerebrum. The behaviorist considers the brain as one organ of the body, granted far more complex than any other, but not specially endowed with qualitatively different powers. The purpose of the stomach is to secrete certain chemical substances and to churn the food in such a way that these gastric juices can get at the food more readily. So, the purpose of the brain is to correlate sensory and motor activities and to carry on entirely central implicit processes.

Another assumption underlying the more positive exposition of Behaviorism is its strongly environmental emphasis. This comes both from its discarding the soul and innate ideas, and from empirical observations on new-born infants. This assumption influences thinking on several topics: learning, intelligence, instinct, and emotions, among others.

The stand on the first topic is largely answered by the premise itself. Heredity accounts for little more than physical structure and a few largely random reflexes. Therefore, learning must be all-important to account for the vast differences between adult and infant.

Intelligence is belittled by Watson, although other behaviorists treat favorably this important topic. Watson's statement, perhaps put strongly deliberately, that he could take any normally formed infant and by controlling the environment make him successful in any designated profession, precludes much importance for intelligence. The writer feels that Watson has ignored a large body of facts here; the constancy of the IQ is a strong argument for heredity. Assumption of a fundamental innate neural plasticity, plus giving all due credit to sensation and learning, would alter the systematic position very little and would be more in line with the facts.

Instinct must be completely discarded by a behaviorist, since it is not only an innate trait, but it involves the accep-

tance of faculties the nature of which it is difficult to define and explain on a physiological or neurological basis. Whatever is innate is included in the list of random reflexes which are present at birth.

Emotions are treated in much the same way. Complete objectivity is necessary, which does away with the introspective approach characteristic of practically all writers from the Greeks to the present, not even excepting the James-Lange theory. The behaviorist's theory is similar to his view on instincts; there are only a few native emotions, which are indefinite mass reactions, and one learns specific situations in which to express the emotions.

The fundamental assumptions of other schools might be examined with much the same results. In the Gestalt school, for example, the magic word Configuration is employed to account for all manners of activity. It originated from certain perceptual facts which are obviously correct in that field, and which have partial confirmation from certain physical phenomena. Insight or intelligence, learning, and attention are occasionally mentioned, but always in terms of configurations. To the reviewer their manner of exposition suggests that rather than expand their general principles into other fields, they have warped those fields into perceptual terms, thus playing into their own hands. Thus insight represents sudden formation or appreciation of a configuration; learning is building up a configuration; instinct is reacting to a configuration which was natively formed.

Thus we see how the acceptance of a fundamental premise influences one's theories along many lines. With this in mind we may examine critically the arguments of Eclecticism.

VI. Evaluation of Arguments

If one grants the logic of our arguments, he will have to accept some existent school of psychology or form one of his own, so arranged that the fundamentals will agree consistently among themselves.

One can not decide the question on the basis of fact. Each school is correct; one can not deny the truth of objective data.

Each may have certain limitations, and may prejudice results by the problems it studies and the conditions it sets up; but they are all basically sound.

It is a hope that some time in the future there will be only one system of psychology. This of course will not be a system, but will be 'Psychology,' and will include the whole field of our science. This eventual viewpoint will undoubtedly be a comprisal of the various systems which exist now, with perhaps some aspects of others which may be brought out in the future. However, this compromise between systems will have to be done consistently, so that the view on each and any point will not assume any premise which is not followed in dealing with other topics. If one departs from a pure system in dealing with one problem, he must depart to an equal extent in thinking about all related problems.

Such a procedure is not Eclecticism, however. As long as one applies a fundamental premise in exactly the same manner to all topics with which it is partially or wholly concerned, he is following an acceptable system. If one could, for example, effect a compromise between some of the fundamentals of Behaviorism and of Gestalt Psychology, and apply that compromise equally to all fields, he is holding a legitimate and consistent system. The same principle is illustrated by introspection. We all know its pros and cons, and are careful not to carry it too far. But almost all of us use it in some way or another: after-images, color vision, emotions, thinking, or learning. One would have no right to use this method with emotions, even if only in supplementary fashion, and then call somebody else unscientific because he used introspection to study imagery in learning. One should use it in the same manner and to the same degree in dealing with every topic, allowing only for differences in behavior levels.

From quotations we have given one would gather that Woodworth is one of the leading advocates of Eclecticism. But I think that he is far from being an eclectic. He follows a definite system, which is characterized by its consistency. It is classifiable as a Purposive Behaviorism. His approach and many of his views are largely behavioristic, but he has the

purposive or dynamic viewpoint. His 'Dynamic Psychology' expresses as consistent a viewpoint as do books by such recognized systematists as Watson, Weiss, Köhler, or Freud. Once the reader has grasped Woodworth's idea of the dynamic aspects of behavior, he is able to predict very accurately the views taken on each individual topic.

Tolman's system, as expressed in various articles and particularly in his recent book (6), resembles Woodworth's in many essentials. He is fundamentally a behaviorist, but favors a drive theory, using a very much improved type of purposivism. This purpose is used in the same manner when he is dealing with various problems, not applied to some and dismissed as unnecessary or impractical with others.

One does not need to hold a closed system. To do so is a serious mistake, particularly with the uncertainties and rapid developments taking place in our field. One should be willing to modify his theories any time a crucial experiment or an all-inclusive theory knocks the foundations from under old premises and makes the acceptance of new ones imperative. But the basic theories one has now or may hold in the future must be accepted consistently, or he will be violating one of his tenets in whole or in part.

Finally, one does not need to adopt any single existing system in its entirety. He may accept elements from two or more systems, providing only that he does this consistently. The one toward which the writer leans is of this type. It is very close to the Purposive Behaviorism of Tolman and Woodworth. These are largely behavioristic, but find it necessary to introduce motivation. There is, however, no vitalistic element, as in Purposivism or Psychoanalysis. Just where some of the obviously correct facts of Gestalt and Psychoanalysis fit in is uncertain. A system may have to remain open until more evidence and deeper thought takes care of present lack of agreement. But this will not be Eclecticism; it will be an admittedly incomplete system. It will not be complete until all accepted facts are fitted together in agreement.

VII. Summary of Arguments

1. Eclecticism has been urged on students of Psychology, since the various schools are incomplete in one way or another. One is advised to select the best from each school, and incorporate all these views into his system.

2. It is the contention of this paper that such a procedure is logically unsound. Any psychological system, no matter what it is, is founded on certain fundamental premises. These premises must be compatible with each other, and all one's theories must follow them out consistently. Otherwise one is accepting a certain premise (monism for example) only in part and rejecting it elsewhere.

3. Since Eclecticism does not satisfy our requirements for a system, it can not be held as a permanently tenable system. At best one may hold what looks most nearly correct in various topics, and later refine and coördinate his views as his thinking and new facts appear.

REFERENCES

1. Boring, E. G., Psychology for eclectics, Psychologies of 1930, Clark Univ. Press, 115–127.
2. Brown, J. F., A note on Dr. Klein's plea for eclecticism, Psychol. Rev., 1931, 38, 182–5.
3. Klein, D. B., Eclecticism versus system-making in psychology, Psychol. Rev., 1930, 37, 488–496.
4. McGeoch, J. A., The formal criteria of a systematic psychology, Psychol. Rev., 1933, 40, 1–12.
5. Smith, S., The schools of psychology, Psychol. Rev., 1931, 38, 461–473.
6. Tolman E. C., Purposive behavior in animals and men, Century, 1932, 463 pp.
7. Woodworth, R. S., Contemporary schools of psychology, Ronald, 1931, especially Chapter 7.

A GESTALT CRITIQUE OF PURPOSIVE BEHAVIORISM

BY WALTER A. VARVEL

University of Kansas

The purpose of this paper is to indicate certain differences between Dr. E. C. Tolman's purposive behaviorism [1] and Gestalt psychology. Superficially the two systems appear to have much in common. However, it is our conviction that isolated points of resemblance are of relatively little significance; for parts derive their meaning from wholes, which govern them, and as members of different systems they function in each case quite differently.

Dr. Tolman is sympathetic toward certain phases of Gestalt psychology. He recognizes that previous attempts of systematic interpretation have left much to be desired and that Gestalt psychology seems to point the way toward a more adequate treatment of psychological problems. Therefore, he is content to label his doctrine of learning "a sub-variety of the Gestalt doctrine" and on the same page to say, "Learning was and is the fundamental keystone of the system." [2] In short, from a configurational standpoint, there is much to be commended in this book. The wealth of experimental work presented would alone justify its study. One will not always agree with Dr. Tolman's interpretations; and, worst of all, one will never be quite certain that he fully understands Dr. Tolman's terminology. Even the author admits a distaste for most of the neologisms he has introduced.

Systematically, purposive behaviorism is an eclecticism. It attempts to reconcile purposivism and behaviorism with

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Gestalt psychology. The first two of these appear irreconcilable, but Tolman asserts, "our psychology is a purposivism; but it is an objective, behavioristic purposivism, not a mentalistic one." [3] Thus, the behavioristic tradition results in a constant struggle to avoid the pitfalls of 'mentalism.' Before discussing conscious awareness, he finds it necessary to apologize for the "shameful necessity for raising the question." [4] He differentiates between mentalism and 'behavior.' The mentalist supposes that organisms really have 'inner happenings.' The behaviorist conceives of mental processes as "inferred determinants of behavior" and defines them in "purely objective terms." "Mental processes are . . . but dynamic aspects, or determinants, of behavior. They are functional variables which intermediate between environmental stimuli and initiating physiological states or excitements, on the one side, and final overt behavior on the other."

Has Tolman succeeded in avoiding the 'mentalism' to which he objects? It would seem that he has merely substituted terms without regard for their inevitable implications. He admits the existence of mental processes again and again. They are 'functionally defined aspects' which determine the animal's adjustments; they are 'purposes,' even 'cognitions'; they give to behavior its 'docile' character; they are actually 'in-lying'; they are 'immanent'; they are 'judgmental' in character; by their means, goals are set up and anticipated; animals possess 'consciousness-ability, ideation ability, creativity.' And finally we read about sensations, images and consciousness (these terms are used) as if they actually did exist. Why deny the existence of phenomena when, in order to avoid them, one must at first constantly employ new terms within quotation marks, and then finally, accept them outright? The difference between Tolman and the modern 'mentalist' is only a verbal one. Terms like sensation, thought, and feeling are not objectionable in a monistic scheme. They are redefined, however. They represent phenomenal properties of an energy system, just as do terms

[3] T, 423.
[4] T, 204.

like heat, sound, light and electricity, without assuming a mind-matter dichotomy, or even a subject-object dualism. The problem which behaviorism faced, in attempting to rule out consciousness was one of its own invention, in terms of its ultra-naïve philosophy. It is a simple matter to avoid the problem by beginning with monistic, rather than with dualistic assumptions.

More specifically, let us consider the problem of purpose and cognition. How are these to be defined in the purely objective terms of behaviorism? Unlike the earlier behaviorists, Tolman does not banish purposes and cognitions from the field of psychology. He admits them as 'functionally defined aspects' since "Purposes and cognitions are of its [molar behavior's] immediate descriptive warp and woof." [5] To Tolman, purpose is revealed and defined by the objective fact of an animal's persistence toward a goal: "The doctrine we here contend for is, in short, that wherever a response shows docility relative to some end, wherever a response is ready (*a*) to break out into trial and error and (*b*) to select gradually, or suddenly, the more efficient of such trials and errors with respect to getting to that end—such a response expresses and defines something which, for convenience, we name a purpose." [6]

Cognition is revealed, in part, by the disturbed behavior observed when a goal-object has been changed (*i.e.* a rat or monkey will show a 'searching' behavior under these circumstances). "Our contention will be that the characteristic patterns of preferred routes and commerces-with which identify any behavior-act can be shown to be docile relative to, and may pari passu be said cognitively to assert: (*a*) the character of the goal-object, (*b*) this goal-object's 'initial position' (*i.e.*, direction and distance) relative to actual and possible means-objects, and (*c*) the characters of the specifically presented means-object as capable of supporting such and such commerces-with." [7] Both of these may readily be seen *in* behavior. They are "the last steps in the causal equation

[5] T, 12.
[6] T, 14.
[7] T, 16-17.

determining behavior," and in their turn are "caused by environmental stimuli and initiating physiological states."[8] They are 'immanent determinants.' The cognitions are subdivided into means-end-readinesses, "which determine the selective responsiveness of organisms to stimuli"[9] and into expectations having the three fundamental modes: perception, mnemonization, and inference.[10]

Such a medianistic approach is characteristic of Tolman's psychology. Why is it necessary to infer back to the animal for purposes and cognitions and capacities? If we do this, we are well on the way to accepting them as mentalistic entities. It is the problem that atomism has always faced. Atomistic psychology is forced either to abandon such concepts as purpose and cognition or to *endow them with vitalistic force*. In the latter case, we have a vitalism which finds in purpose an innate driving force that propels an organism toward a goal. We do not avoid the problem by resorting to 'functional definitions.' We can not 'invent' objective variables and assign to them such power as Tolman gives them.[11]

Gestalt psychology rejects such a one-line-functionalism,[12] such an explanation from point to point in terms of independent entities or innate forces, in favor of a multi-line-functionalism operating under laws of dynamics. All points in a system, field or pattern, are necessary to explain the contin-

[8] T, 19.

[9] T, 451. Note also: Tolman and I. Krechevsky, Means-end-readiness and hypothesis: A contribution to comparative psychology, PSYCHOL. REV., 1933, 40, 60–70. Here means-end-readiness is defined as "a certain selectivity as regards stimuli, and as regards the responses to be made to such stimuli, which the animal, because of innate constitution or previous training and as a result [of] his physiological demands at the moment, brings with him to any given stimulus-situation." This will indicate the relation between what Tolman calls the "four causes of behavior": stimulus, heredity, training and initiating physiological state, which are regarded as causal conditions responsible for the appearance of means-end-readinesses and hypotheses. Means-end-readiness refers to a wider and more general level of selectivity. Krechevsky's 'hypothesis,' on the other hand, refers to a "narrower, more specific selectivity appearing . . . within the range of this wider selectivity."

[10] T, 444.

[11] T, 422.

[12] R. H. Wheeler, F. T. Perkins, & S. H. Bartley, Errors in recent critiques of Gestalt psychology I. Sources of confusion., PSYCHOL. REV., 1931, 38, 109–136. See especially 127–128.

uity and direction of a given event. Purpose is a field property of a conscious whole. There is no independent source of energy inherent in a purpose any more than there is weight *in* a rock or *in* a table. The purpose of which we speak, as a field property, represents a *structured* field of energy. A high energy potential exists in a field in reference to a low. The goal and the line of resolution are both simultaneously conditioned by the immediate situation.

Cognition, likewise, is envisaged in Gestalt psychology under the concept of insight, which again is governed by laws of a dynamic whole. Purpose and cognition are not *in*, they are *of* the organism. They are field-properties, sustained by the organism's relation to its environment. Tolman sees this only in part. The same objections apply to his postulation of capacities. Such postulations give us no hint of basic organization. Until these are considered not as 'immanent determinants' but as 'derived properties,' psychology will remain in a blind alley. The traditional dualistic approach set for psychology an impossible problem. Behaviorism accepts the dualism, although it attempts to avoid one side of the dichotomy.

Let us consider for a moment Tolman's behavior determinants.[13] They are divided into: (1) Capacities regarded as ultimate genetic and training factors. Relatively minor, independent 'funds' "may add together in all sorts of complicated, varying and overlapping ways to produce the actual discrimination, manipulation, means-end-successes." (2) Capacities considered as response requirements of actual S-R sequences. They are roughly divided into capacities for certain sensations, for certain behaviors, for attaining goal-objects in a certain way, for retention, for consciousness and ideation, and for creativeness. Grouped in such a fashion, they are analogous to Spearman's general and specific factors.[14] (3) Purposes and cognitions, subdivided into demands, means-end-readinesses, discriminanda- and manipulanda-expectations, means-end-(sign-Gestalt) expectations. (4) A substi-

[13] T, 412.
[14] T, 408.

tute for overt trial and error behavior, by means of which an animal attains a sampling of alternative responses.

Such concepts as the above are not new to psychology, but in this form they are most decidedly new to a behaviorism. If they have any significance, it can be only from a descriptive standpoint. Such hypotheses do not lend themselves to experimentation; they can never be proved. They remain only as *inferences* from behavior. They postulate the actual causes of behavior as discrete 'funds.' This is essentially the Aristotelian concept of dynamics.[15] There the vectors determining an object's movement were considered as belonging to the object itself, irrespective of its surroundings at any given time. Today it is realized in physics that the dynamics of an event may be determined only in reference to the concrete whole comprising both object and situation. Both the Aristotelian and the modern conception of vectors deal with directed forces (in psychological terms, with 'purposes,' 'strivings,' and 'drives'); yet in the first the vector is a property of the object itself, while in the second the vector is a field property. It is this distinction that we believe Tolman overlooks when he compares his use of purposive concepts to Kurt Lewin's use of purpose.[16] The one approaches psychology from an atomistic point of view; the other from a relativistic, functional point of view. Capacities regarded as ultimate genetic and training funds have no significance in relation to specific situations. A variable and adaptable capacity is very much like Thorndike's connectionism when generalized to deal with total situations.[17]

The problem of the relation of fact and hypothesis is another systematic heritage confronting Tolman as a purposive behaviorist. He says: "A purposive behaviorism agrees with a strict behaviorism in asserting that organisms,

[15] K. Lewin, The conflict between Aristotelian and Galileian modes of thought in contemporary psychology, *J. Gen. Psychol.*, 1931, 5, 141-177.

[16] See also R. H. Wheeler, Laws of human nature, 1931, 49.

[17] E. L. Thorndike, Human learning, Century, 1931. "A very common type of connection is one in which the situation produces the response of doing whatever is adequate to attain a certain goal, rather than . . . to one particular movement or idea or series of such," p. 92.

their behavior and the environmental and organic conditions which induce the latter, are all that there is to be studied. . . . Stimuli and responses and the behavior-determinants of responses are all that it finds to study." [18] As a bare statement of one's position with respect to psychological data, few modern psychologists would object to it. However, it is a different matter when one attempts, under such a formula, to rule out explicit consideration of philosophical problems basic to science. We do not free ourselves from 'mysticism' merely by ignoring philosophy. We can by no means agree with Dr. R. M. Elliot, who, in introducing Dr. Tolman's book, writes "He has given us not only a complete psychology but a behaviorism which is neither physiological nor metaphysical. It is a thoroughly intrabehaviorial system, dependent on nothing outside itself except experimental observations of animal and human behavior." [19]

A study of Dr. Tolman's system will not substantiate this statement, though, as a methodological dictum, Dr. Tolman would unquestionably agree with it. This but reasserts the traditional empirical position. Pure empiricism is a scientific impossibility. There are no purely objective scientific facts, facts which do not contain or depend upon an assumption or inference. Experimental observations are not self-interpretative. Why was Tolman compelled to 'infer back from behavior'? His whole system of 'intervening variables between stimulus and response' rests upon inferences; they *were not* and *can not* be 'experimentally observed.' Scientific methodology is and must, of necessity, be more than observation. There must be a critical examination and definition of concepts employed. By means of a self-consistent framework of logic particular details become significant. An hypothesis is developed as to the nature of the phenomena under question. This hypothesis is then checked by means of a critical experiment expressly designed to test its validity. As Kurt Lewin has suggested, such a constructive method is essentially the method by which the science of physics has progressed;

[18] T, 417-418.
[19] T, vii.

it must be the method employed by psychology, not because physics is in any sense basic to psychology, but because the method of science has been best exemplified in the work of the great physicists. Science cannot be divorced from philosophy and logic. Since our observations are in large part dictated by our assumptions, the hypothesis must be shown to have both a logical and a factual consistency. "Hypothesis non fingo" was never more than a methodological figment. Atomism or 'machinism' broke down through internal inconsistencies and self-contradictions.[20]

Tolman's distinction between molar and molecular behavior arises from his belief that behavior of the whole organism is a derived rather than a primary fact. "It is a mere corollary of the more fundamental fact that behavior *qua* behavior, as molar, is docile and that successful docility requires mutual interconnection between all the parts of an organism."[21] He points out that even John Watson has dallied with these two notions of behavior: molecular behavior, which is behavior defined 'in terms of its strict underlying physical and physiological details, *i.e.*, in terms of reception-process, conductor-process, and effector-process *per se*'; and molar behavior which 'is more than and different from the sum of its physiological parts.' Such behavior, as an emergent phenomenon, has 'descriptive and defining properties of its own.' It is this type of behavior which is of paramount interest to psychology.

The descriptive and defining properties of molar behavior are: (1) Behavior "always seems to have the character of getting-to or getting-from a specific goal-object or goal-situation." (2) This "always involves a specific pattern of commerce-, intercourse-, engagement-, communion-with such and such intervening means-objects, as the way to get thus to or from." (3) "Behavior-acts are to be characterized, also, in terms of a selectively greater readiness for short (*i.e.*, easy) means activities as against long ones."[22] With some modifications, these descriptive properties correspond to the Ges-

[20] R. H. Wheeler, Laws of human nature, 1931, 37f.

[21] T, 19.

[22] T, 10-11.

talt terms of goal, pattern or configuration, and least action. However, there is a fundamental difference between Tolman's view of molar behavior and the Gestalt view of behavior. Tolman still attempts to derive wholes from their parts. Wholes are not primary. Unity is secondary, after all, while for Gestalt psychology it is primary.

Dr. Tolman has attempted to avoid the philosophical problem of emergence. When he speaks of 'emergent properties,' he claims to be using this term in a purely descriptive sense and not to be taking sides in philosophical controversy. "Emergent behavior phenomena are correlated with physiological phenomena of muscle and gland and sense organ. But descriptively they are different from the latter. Whether they are or are not ultimately in some metaphysical sense completely reducible to the latter we are not here attempting to say." [23]

But the problem is so vitally significant to psychology that it cannot be so lightly pushed aside. We must and we do answer it, whether our answer be logically explicit or whether it be only implied. Tolman's treatment of the whole question of molar and molecular behavior implies a particular answer, and it is the answer of atomism-synthesis. "Behavior as such cannot, *at any rate at present*, be deduced from a mere enumeration of the muscle twitches, the mere motions *qua* motions, *which make it up*. It must *as yet* be studied first hand and for its own sake. . . . These new properties, thus distinctive of molar behavior are presumably strictly correlated with, and, if you will, *dependent upon*, physiological motions. But descriptively and per se they are other than those motions." [24]

[23] T, note 9, p. 7.

[24] T, 8 (italics ours); Tolman's reply (*Psychol. Bull.* 1933, 30, 459–465) to Koffka's criticism (*ibid.*, 440–451) of this concept of emergent behavior is chiefly devoted to an attack upon the validity of the principle of isomorphism. Tolman admits the existence of two series, a brain-physiology series and a behavior series, but maintains that the latter is predictable from the former only after the empirical discovery of transformation equations between the two. A dualistic position seems to have created the difficulty here. He admits that his treatment of brain physiology as molecular 'if implied, was a slip-up.' That the implication was made can hardly be doubted. The admission of error does not destroy the pertinency of our objection, since throughout his system he continues to employ the concepts of synthesis and derived unity.

The distinction seems to be little more than that referred to in the principles of creative synthesis. Something is obtained from nothing, unity from chaos. The philosophical problem does not appear to concern Tolman; the methodological problem most decidedly does. He recognizes that behavior, considered as such, possesses molar properties; that is, the behavior of the organism appears to be unified, to be directional and purposive in character, and to be adaptable. Such behavior cannot be described in terms of the muscle twitches 'which make it up.' His position is not consistent. He recognizes that behavior is molar, *but he is not prepared to account for it in molar terms.* He assumes that the conditions for such behavior are unorganized and chaotic. He attempts to describe purposeful behavior occurring in a field that is not purposeful but rather completely out of relation to such behavior. He is forced to resort to heredity and training, to innate funds and capacities and appetites to give behavior the dynamic qualities it undeniably exhibits.

Tolman constantly emphasizes that his system "concerns itself with, and is valid for, docile behavior only." [25] The fact of 'docility' ('teachableness') is his major evidence for the existence of molar behavior. Only molar behavior exhibits docility. Therefore, "In so far as behavior is not docile, but goes off willy-nilly by virtue of invariable reflex stimulus-response connections, a description of it in terms of immanent sign-Gestalt-readinesses and -expectations and hierarchies of demands, and the like, would be both silly and meaningless." [26] Furthermore, "The lower the organism, or the more internal and physiological the response, the more likely, it would seem, that a given act is non-docile, *i.e.*, of a purely reflex or tropistic variety." Though admitting some evidence for modifiability far down the animal scale, he seems to accept without question the existence of two kinds of behavior, molar and molecular. This point is clearly revealed during his discussion of the conditioned-reflex theory of learning. He criticizes it as an adequate theory 'at our level of interest' (molar behavior), but

[25] T, 414.
[26] T, 415.

concludes: "This does not deny, however, its possible validity as a principle for the physiological units underlying behavior. It may still be that the phenomena of sign, of expectation, etc., which we find involved in learning, described at the level of behavior *qua* molar, may be built upon, or out of, conditioned-reflex sorts of connections among the elementary physiological processes which underlie behavior." [27] He suspects, however, that even here the mechanism may not be so simple as many have thought. Be this as it may, the principles of Dr. Tolman's 'complete psychology' are far from 'complete.' He is now denying molar behavior as fundamental. It is mechanically derived. He has forsaken his approach to Gestalt psychology. How much better is this than all of the atomisms we have had before?

Tolman believes that the part is primary and of an elemental nature. Though he speaks of 'Gestalts' he is much closer to the Gestalt-qualität conception of the whole than he is to the modern Gestalt position. This, we believe, is the basic point of variance between purposive behaviorism and organismic psychology. Failure to recognize this distinction of whole-part relationship permeates the entire system. In line with its view of the Gestalt as primary rather than as a fusion of part-variables, organismic psychology accepts organization as a principle of general application. Instead of discrete elements for whose organization we must account, we assume the whole to be primary and the parts to be derivatives from the whole. The parts, therefore, exhibit certain characteristics only by virtue of their membership character in the whole. A part considered as such, has no explanatory significance. The supposedly discrete movements which we 'see' as part-movements arise through differentiation from an originally homogeneous field. The so-called reflex is the end stage of a given growth process, through differentiation, not the first stage of a building-up process. There can be no conditioned-reflex 'theory' of learning, though certainly there are conditioned reflexes. Our principles of explanation must be in terms of organized wholes, not in terms of interconnec-

[27] T, 336.

tions between parts. The fact that behavior is adaptable does not imply "mutual interconnections between all parts of an organism." [28] Rather it means that an organism always stands, as a unity, in the closest relationship to its environment that, indeed, neither can be regarded as separate. Under laws of the whole, differentiation and maturation are constantly taking place.

Tolman compares Gestalt psychology to American structuralism. He believes: (1) that both are normative or 'stimulus' psychologies; [29] (2) that neither explain how intervening mental processes "go over into and cause behavior." [30] He believes Gestalt psychology is an improvement over structuralism in that: (1) "it has better and more detailed description of the M_c variables" ['perceptions,' 'meanings,' 'purposes,' etc.]; (2) "it has better laws ['figure-ground,' 'grouping,' 'closure,' 'Prägnanz'] than the old too simple laws of association as to how the M_c variables arise out of the M_a variables" ['sensations,' 'images,' and 'affections']; (3) "it brings out the fact that the M_a variables are really only functionally defined entities and never have actually an independent existence in and by themselves." [31]

What shall we say of such criticisms and left-handed compliments? First, in charging that Gestalt psychology assumes but one final cause underlying behavior—the stimulus—Tolman is neglecting the Gestalt emphasis upon such factors as maturation and non-process condition. The organism develops and differentiates as an organized whole. It always behaves in conformity to the same general laws, but the particular form behavior assumes at any one time is determined in part by a particular stage of maturation or development, that is, by internal, dynamic organization. Maturation takes

[28] T, 19.

[29] *See also* Krechevsky, Hypothesis in rats, PSYCHOL. REV., 1932, **39**, 518, note 3: "The Gestaltists, it seems to us, lay themselves open to the same criticism that they make of the 'connectionists.' If one reads 'stimulus-configuration' for 'stimulus' and 'Gestalt of responses' for 'response,' one can formulate the Gestalt hypothesis of S-R relationship in the very same way as the strictest behaviorist."

[30] T, 400.

[31] T, 399.

place within certain limits determined by 'heredity,' and by the stimulus-patterns now functioning. Considered as two separate factors, heredity and environment are meaningless concepts. Tolman has, once more, missed a vital point, namely, the relativity aspect of Gestalt psychology. There is never only one cause or type of cause. Causation is multiple and comes from the unified whole of which a given event or object is a part. The picture changes when reference points change. Maturation of structure, when considered external to learning, is a cause of learning, along with stimulation. When included in the same pattern of effects as learning, the growth potential and stimuli are the causes.

Moreover, Tolman's belief that the Gestalt-stimulus relation is similar to the behaviorist's conception of stimulus and response is founded on a serious misapprehension of Gestalt psychology. First, in the behaviorist's view, nothing is said of the dynamic relation of the organism to its environment, nor are the consequences faced of the existence of this relationship. Arrangements of stimuli 'form' or 'align' energy patterns within the organism, but, in turn, under the law of action and reaction, the organism is *active*. It is aggressive under restraint or compulsion. Second, the effect one stimulus will have depends upon the effects other stimuli are having. Third, no stimulus acts upon a single part of the organism. The organism is a fluid field responding as a dynamic unit to a total situation. The response of the whole organism is 'a single piece.' Fourth, there is nothing in the picture anywhere of the nature of a 'bond.' The S-R relationship is in the nature of two stages in a continuous process, a resolution of a differential in energy potentials between the organism and its environment. Indeed, all down the line, a better understanding of Gestalt principles could be obtained if they were not compared with those of other psychologies, but examined for their unique, internal form. Comparisons lead only to an obscuring of the principles.

Tolman's charge that Gestalt psychology does not explain how intervening mental processes 'go over into and cause behavior' raises again the problem of dualism. The behavior

of the organism may not be divided into a series of independent stages. Stimulus cannot be separated from perception without destroying the total structure of the process in question. Perception and overt action are phenomenal aspects of a more basic functional unity, and, as such, are abstractions from a whole that, unanalyzed, is neither physical nor mental, but *dynamic* and *structural*. One does not *cause* the other nor *flow* over into the other. Stimulus, mental process, response —these are not separate and distinct. Mental processes do not bridge a gap between stimulus and response; they are parts of an all-inclusive, dynamic field and have no meaning except in terms of that field. We need no production-processes in Gestalt psychology; neither do we regard the intimate relationship between perceptional and behaviorial patterns as a problem. Whether we choose to select a mental phenomenology or a physical one, with which to deal, we are involved in *Gestalten* and in the laws of dynamics. In short, when the organism thinks and moves, it is responding as a unity, and it is action in either case. There is no distinction in terms of space and time, between mental and physical action. The organism, unmolested by analysis, is *not* a *psychophysical* organism, but *an organism* from whose total reaction 'mental' and 'physical' aspects can be abstracted. If one chooses to employ the term 'mental' to describe the unanalyzed response, that term *includes* the physical. It is descriptive of the *whole* reaction as it originally stands.

Tolman misinterprets a major principle of Gestalt psychology when he speaks of the laws of Gestalt as attempts to explain how Gestalts ('mentalistic entities') *arise out of* sensations, images, and affections. These are not laws attempting to account for organization. Tolman's error is almost inexcusable! It is his belief that a denial of such entities "when properly understood, is a denial that these latter ever exist separately and in their own right—uncaught-up into Gestalts." [32] The last phrase is completely incorrect; Gestalten are never formed by a building-up process. He continues: "The Gestalt psychologies do, that is, assume M,

[32] T, 399.

variables in the sense of the ultimate possibilities of sensory discrimination and of motor manipulation. These . . . are functionally, the data upon which their laws of closure, grouping, figure-ground, etc., operate. But these possibilities have no existential being either prior to or independent of the full-blown Gestalts which they mediate." A most curious statement. The first half of the last sentence is correct; the second half ('which they mediate') is incorrect, and is Tolman's own error, not a Gestalt position. Parts do not mediate wholes. Tolman is reading atomism into a non-atomistic position. The logical items are parts and wholes, and wholes come first in any genetic or derivative process. Experience is not *composed* of elementary processes. The problems are those of unity and structure, not of analysis and composition. Tolman is insisting upon regarding the sensation, image, and feeling tone as pre-existing elements. In Gestalt, parts are derived from wholes by differentiation; wholes are not derived from parts by means of synthesis. That which Tolman puts last, Gestalt psychology puts first, and *Tolman says not a word about the reasons why.*

Tolman states: "the fact of some sort of a 'togetherness' as the essential condition for the formation of 'organization' (Gestalts) is perhaps the main burden of the contributions of Gestalt psychology to the problems of learning and memory." [33] He accepts outright Thorndike's use of 'belongingness' or 'togetherness.' As two laws of learning, he advances (*a*) the Law of Togetherness, "Some sort of temporal and spatial, or other 'togetherness,' [is] essential to easy 'Gestalting,'" (*b*) the Law of Fusibility which "adds the further point that certain types of 'qualitative togetherness' will also make for easier Gestalting than will others." [34] These are not 'Gestaltlike laws.' There is no 'Gestalting.'

By his use of such terms, Tolman is imposing upon Gestalt psychology concepts foreign to its whole orientation and significance. The problem of the relation of whole and parts is one of the most vital problems of modern science. The

[33] T, 381.
[34] T, 381.

Gestaltqualität school made this problem explicit for psychology, but their attempted solution was not fundamentally different from older theories. Gestalt psychology brought to this problem a new point of view. Let Koffka state it for us.[35] "Neither substance, nor energy, nor events prove to be divisible, ad libitum. Therefore, instead of starting with the elements and deriving the properties of the wholes from them, a reversal process is necessary; i.e., to try to understand the properties of the parts from the properties of the wholes. The chief content of Gestalt as a category is this view of the relation of parts and wholes involving the recognition of intrinsic, real, dynamic whole-properties." As Koffka points out, such a view will alter Hume's concept of causality, "which attempted to reduce causality to mere factual sequences of point events."

Tolman's doctrine of learning serves to illustrate his misconception of the character of the Gestalt. He states, "the learning trials function to determine and define a total complex—shall we say 'Gestalt'?. . . The process of learning any specific maze is the building-up of, or rather a refinement of and a correction in, the expectations of such specific wholes . . . or, as we shall hereafter call them, sign-Gestalts." [36] Quite obviously there is a discrepancy between the terms 'building-up of' and 'refinement of and a correction in.' These represent two contradictory positions, the one starting with parts, the other with complex wholes. The first seems to correspond more closely to his general position. However, an interesting point arises when he discusses the question: what would happen if the stimuli for each of the parts of a possible sign-Gestalt were immediately present? Tolman considers the theoretical possibility: "we assume that here, as in the case of a mnemonization, there will also be some degree of fusion between sign, significate, and signified means-end-relations. In case of the perception, however, the fusion will, by definition, result net from any past experience; rather it is to be conceived as prior to any specific experience

[35] Encyclopedia of the social sciences, 1932, 6, 642-45.
[36] T, 135-136.

of the successive aspects of the whole complex. . . . The fusion must be conceived therefore as coming directly, insofar as it does really obtain, from the interarrangements of the stimuli themselves." [37]

This is an important assumption, inasmuch as here no experience is necessary to accomplish such unification. The interarrangements of stimuli, themselves, tend, as a whole expectation, to evoke the unified sign-Gestalt before any movement of the animal in the maze occurs. *The situation is perceived as a whole.* Here the *Gestalt* seems, in Tolman's own words, to be something other than a derivative. If a perception of the whole comes directly from a dynamic relation to a total situation, the term "fusion" is an anomaly. But 'inter-arrangements' is a dangerous term. It implies that the organism is responding to a *Gestalt*. It isn't. It is responding to a multiple-stimulus situation. The physical stimuli, *among themselves*, are Gestalten, not Gestalten with respect to the responding organism. The organism, in reacting relationally to the multiple stimulus, not its relations, exhibits *Gestalten*.

Tolman finds it necessary "to look *behind* and *within* the Gestalts to independently distinguishable variables to be treated as the determiners and components of such Gestalts." [38] Why does he find it necessary? Only because, for him, explanations run from parts to wholes, medianistically. Only because, starting with Tolman's assumption, it is necessary. But what of the assumption itself? Is it necessary or correct? Tolman does not face these issues. Structural analysis is permissible, but we must not forget that the whole is thereby destroyed. We may not assume that a description of such analyzed parts will suffice to explain the whole which is experienced. Structural analysis is not denied by Gestalt psychology. It is merely denied that when the parts have been 'torn out' they can either logically or actually be put back together again. The problem is that of studying the part *in the whole*, not separated from it. The procedure is

[37] T, 137.
[38] T, 420.
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functional analysis, that upon which prediction is based, with mathematical equations, or their equivalent, as the instruments. Tolman has not concerned himself adequately with the larger problems of prediction and control as seen from the Gestalt viewpoint. However, in spite of the many serious distortions of Gestalt psychology to be found in Tolman's book, as an effort to test out a working hypothesis systematically by a long series of experiments, it is an outstanding and monumental piece of work.

SUMMARY

Purposive behaviorism attempts eclectically to reconcile an objective but atomistic behaviorism with the dynamic aspects of an organismic purposivism. It rests, therefore, upon basically contradictory assumptions.

First, Tolman compromises with 'mentalism' more perhaps than he would care to admit. Involved in an unnecessary dualism, he meets serious difficulties both of procedure and of vocabulary.

Second, Tolman reduces the dynamic pattern proposed by Gestalt psychology to a mechanistic pattern. This is precisely the procedure, which it is the primary aim of Gestalt psychology to avoid. He begins with parts and regards organization and unity as derived properties, while Gestalt psychology accepts them not only as facts, but as principles and therefore proceeds in the opposite direction, from the whole to the parts. Tolman has missed the irreconcilability of these two procedures.

Third, the conflict in the Tolman system arises out of the supposition of two kinds of behavior—molar and molecular. While he recognizes the necessity of dealing with molar behavior, his atomistic position prohibits him from treating unified, directional and adaptive behavior in molar terms.

Fourth, despite the partial recognition of unity and field structure, the variables with which he attempts by definition to deal are not field properties but discrete 'funds' and 'capacities,' possessing innate instead of derived properties. His system, therefore, fails to avoid the mechanistic inadequacies that Gestalt psychology purports to avoid.

Fifth, purposive behaviorism as presented by Tolman distorts other major conceptions of Gestalt psychology. His identification of Gestalt psychology with 'stimulus' psychologies neglects the Gestalt emphasis upon maturation and growth potentials and overlooks configurational interpretation of causation. Further, his atomistic assumptions lead him to characterize as 'Gestalt-like' such laws as the so-called 'Law of Fusion,' which is diametrically opposed to the very basis of Gestalt psychology. Such a law presupposes the primacy of parts, while Gestalt psychology is based on the dominance of the part by the whole. Tolman has missed the fact that Gestalt psychology substitutes differentiation and closure for synthesis. It may be that he approaches the Gestalt problem, but he represents the Gestaltqualität position rather than the modern Gestalt position.

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NOTE

A SUGGESTION FOR MAKING VERBAL PERSONALITY TESTS MORE VALID

BY SAUL ROSENZWEIG

Psychological Clinic, Harvard University

In a previous communication [1] an attempt was made to define and analyze the so-called 'opinion-error' in psychological experimentation. As there defined, this error arises whenever the subject 'entertains opinions about the experiment—what its purpose is and what he may reveal in it—instead of simply reacting in a naïve manner' (p. 343). Though controlled experimentation was the central problem of the previous paper, it was pointed out incidentally that the opinion-error enters as a considerable factor in tests of personality by such methods as the questionnaire. The present note is an attempt to describe this briefly mentioned difficulty more fully, and suggest a remedy for it.

An individual's statements about his own traits are seldom very reliable. There are several possible reasons for this: (1) the subject's ignorance of his own personality: this may consist in either (*a*) an absence of knowledge or (*b*) the presence of an active resistance to the recognition of certain facts about one's personality—a process that may go on even without the subject's being much aware of it; (2) the subject's unwillingness to admit certain facts about his personality to others. These processes have often been described under the terms 'lack of insight,' 'self-deception,' 'rationalization,' and 'repudiation.'

Despite these facts, one of the most commonly employed methods of measuring personality traits at the present time is that of the questionnaire, in one form or another. Those who have devised and used such tests have certainly not been unaware of the above difficulties but they have seldom been able to do much about them. Astute phraseology in the instructions and questions of the test have sometimes been resorted to, but such expedients are rarely very effective. Might it not be more effective to recognize at the outset that such tests have certain limitations that can never be completely circumvented and then go on to the measurement of these limiting

[1] S. Rosenzweig, The experimental situation as a psychological problem, PSYCHOL. REV., 1933, 40, 337-354.

factors themselves, thus obtaining information by which a correction may be applied to the subject's answers?   That, at any rate, is the suggestion contained in this note.

Consider the following schema:

A. Actual Traits	*B. Ideal Traits*
1. What a man thinks he is.	1. What a man thinks he would like to be.
2. What a man is.	2. What a man would like to be.

Most personality questionnaires are obviously designed to measure $A.$ 2—what a man is.   Difficulty arises, however, since the subject necessarily answers in terms of $A.$ 1—what he thinks he is— and between this and $A.$ 2 there is usually a discrepancy due, in large measure, to B—what the subject would like or thinks he would like to be.   Now, it is obvious that B could be much more readily measured than A, because B is often a screen or a compensation for A and is thus, in fact, intended for public consumption.   With a knowledge of B, moreover, it might be possible to infer a great deal about the discrepancy between $A.$ 1 and $A.$ 2 and thus approximate much more closely than otherwise to a knowledge of the latter.

The suggestion is thus in order that, in attempting to measure any personality trait by means of a questionnaire, a considerable proportion of the questions begin with the words "I should like to be the sort of man who. . . ."   Such questions would obviously belong to class B and would be answered in terms of $B.$ 1, but there would be much less discrepancy between $B.$ 1 and $B.$ 2 than between $A.$ 1 and $A.$ 2 because the latter discrepancy is largely determined by B factors.   The other questions would be of the usual A type.   One could then obtain two scores for the subject on the trait in question—an $A.$ 1 score and a $B.$ 1 score.   The latter would be interesting in itself for many purposes, but from our present standpoint, its importance would lie in the fact that from it a correction for the answers to the A type questions might be derived.   One could thus obtain a corrected $A.$ 1 score that would be a close approximation to an $A.$ 2 score.   It would not, of course, be a perfect $A.$ 2 score, since a good deal of $A.$ 2 is not concealed by B factors but is simply not known to the subject.   It would, however, certainly be a more valid measure of what a subject is in a given respect than a score based upon uncorrected answers—such as are now relied upon.

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NOTE

The articles by Varvel and Husband were edited by the late Professor Warren and the articles by Bard, Cason, Cook and Rosenzweig were edited by the present editor, H. S. Langfeld.
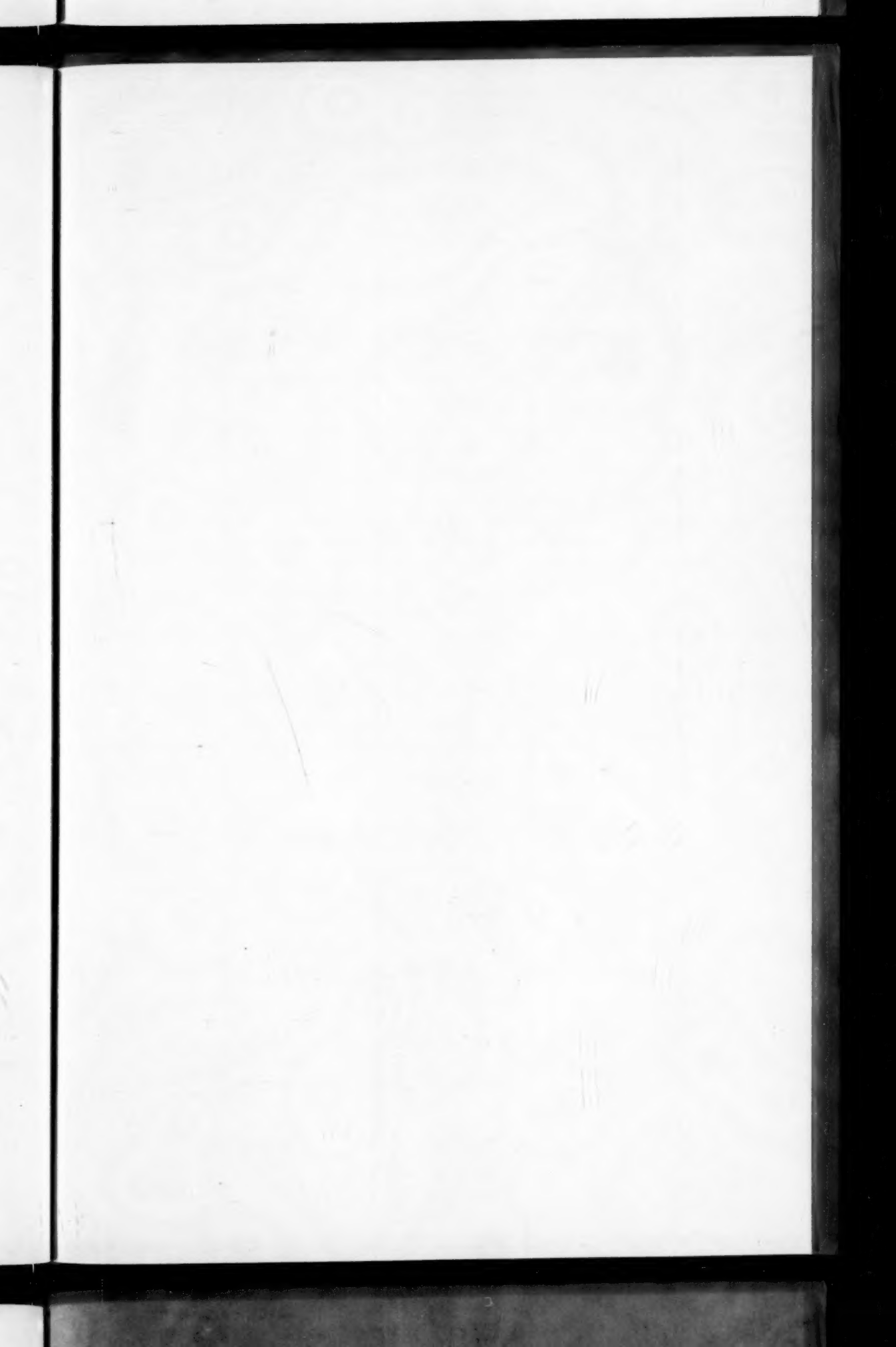
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